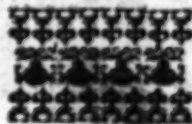


HYDROSTATICAL
PARADOXES,
Made out by New
EXPERIMENTS,
(For the most part
PHYSICAL and *Easie*.)

By the Honourable
ROBERT BOYLE;
Fellow of the Royall Society.



OXFORD,
Printed by *William Hall*, for *Richard Davis*,
Anno Dom. M. DC. LXVI.

HYDROSTATICAL
TARADOXES.

EXPERIMENTS.

PHYSICAL AND MATHEMATICAL.

ROBERT TAYLOR.

LONDON:
Printed by J. Smith, at the Printing Office, No. 10, St. Paul's Church-Yard.
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THE
PUBLISHERS
ADVERTISEMENT

TO THE
READER.

When the Author Writ
the following Treas-
ure, he had a design,
as appears by some
passages in the Pre-
face, to publish to-
gether with it, some
things, which he had divers years be-
fore provided for an Appendix to this

To the Reader.

Physico-Mechanical Treatise about the
Aire: But part of the Appendix
consisting of Experiments, which the
Authour has several times made, but
trusting to his memory, did not think
it necessary to Record, when he came
to recollect particulars, he found that
some years which had pass'd since di-
vers of them were try'd, and variety
of intervening occurrents, had made
it unsafe for him to rely absolutely up-
on his Memory for all the circum-
stances fit to be set down in the Hy-
storical part of the design'd Appendix.
And therefore he resolv'd to repeat di-
vers Experiments and Observations,
that he might set down their Phæno-
mena whilst they were fresh in his
Memory, if not objects of his sense.
But though, when he Writ the fol-
lowing Preface, he did it upon a pro-
bable supposition, that he should sen-
sibly be able to repeat the intended
Tryals,

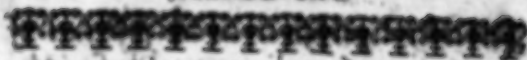
To the Reader.

Tryals; yet his Expectation was sadly disappointed by that heavy, as well as just, Visitation of the Plague which happened at *London* whilst the Author was in the Country: and which much earlier then was apprehended, began to make havock of the People, at so sad a rate that not only the Glass-men there were scatter'd, and had, as they themselves advertis'd him, put out their Fires, but also Carriers, and other ways of Commerce (save by the Post) were strictly prohibited betwixt the parts he resided in and *London*; which yet was the only place in *England* whence he could furnish himself with peculiarly shap'd Glasses, and other Mechanical Implements requisite to his purposes; And the same Calamity continuing still, without yet affording us any certaine ground of determining when it will end: The Author chuses rather to suffer the follow-

To the Reader.

ing Paradoxes to come abroad without
the Appendix, (which is no way neces-
sary to them, whatever they may be
to It,) then any longer put off those In-
genious persons that solicited the pub-
lication of them.

THE



The P R E E A C E.



The Rise of the following
Treatise being a Com-
mand impos'd on me by
the Royal Society, the
Reader will, I hope,
need no more than this

from wondering to find some passages word-
ed as parts of a Discourse pronounced be-
fore an Assembly, it being not unusual
(though not necessary) to present either in
writing or by word of mouth, together with
the Experiments made before that Illustri-
ous Company, and historical account of Them.

But because 'tis probable, that some
Readers will desire to be satisfied about o-
ther particulars, relating to the publication
of this Treatise, I presume it will not be
amiss, both to say something of the Rea-

The Preface.

Tells why I publish it as the first part of the present Appendix to my Physico-Mechanical Experiments, and to give some account of the manner of writing it.

I had quickly both an opportunity and an Invitation to enlarge the papers I was to read, beyond the Limits of a bare description of the Phenomena, and matters of fact, by my having been through some intervening Accidents so hindered from exhibiting them altogether, that I was desir'd to bring in an Accompt in Writing, that might be Registered (how little soever worthy of such Company) in the Societies Collection of Philosophical Papers, for the sake of those Members who could not be present at all the Experiments: So that finding some enlargements expected from me, I was easily inclin'd to add the Explications of the Phenomena I describ'd, whilst I perceiv'd that by a small addition of pains I might much gratifie divers Ingenious Friends that were not so well vers'd in Hydrostaticks as in the other parts of real Learning.

Ha-

The Preface.

Having thus been induc'd to enlarge the Account of my Experiments till it had attain'd the bulk 'tis now arriv'd at, I confess I was without much difficulty perswaded, that to suffer it to pass abroad in the Company of the Appendix wherewith 'tis publish'd; would not prove unacceptable to the Curious, no more then an improper introduction to the rest of my Appendix, and that for several Reasons.

*About this passage,
See the Publisher
to the Reader.*

For (first) the Hydrostaticks is a part of Philosophy, which I confess I look upon as one of the ingeniousest Doctrines that belong to it. Theorems and Problems of this Art, being most of them pure and handsome productions of Reason duly exercis'd on attentively consider'd Subjects, and making in them such Discoveries as are not only pleasing, but directs of them surprising, and such as would make one at first wonder by what kind of Ratiocination men came to attain the knowledge of such unobvi-

The Preface.

ous Truths. Nor are the delightfulness, and the subtilty of the Hydrostaticks, the only things for which we may commend Them: For there are many, as well of the more familiar, as of the more obscure Phenomena of Nature, that will never be thoroughly understood, nor clearly explicated by those that are strangers to the Hydrostaticks; upon whose Principles depend, besides many other things, the Explications of most of the Physico-Mechanical Experiments, we have ventur'd to present the Publick, and the Decision of those many Controversies, which they, and the Phenomena of the Torrecellian Experiment have occasion'd among the Modern Inquirers into Nature.

But the use of this Art is not alone Speculative, but Practical; since not onely the propositions it teaches, may be of great importance to Navigation, and to those that inquire into the magnitudes and gravities of Bodies, as also to them that deal in Salt
works:

The Preface.

works: But that the Hydrostaticks may be made divers ways serviceable to the Chymists themselves, to whose Art that Doctrine seems to be so little of Kin, I might here manifest, if I could think it fit to transcribe, what I have elsewhere deliver'd to that purpose.

* Chiefly, in several places of the unpublished part of the Treatise of the Usefulness of Experimental Philosophy.

But that which invited me to Write something of this part of Philosophy, is, not only that I think it considerable, but that, notwithstanding its being so, I find it but very little, and not very happily cultivated. For being not look'd upon as a Discipline purely Mathematical, the generality of Mathematicians have not in their Writings so much as taken notice of it, much less improv'd it. And since the admirable Archimedes, who, in his little Tract De insidentibus humido, has left us three or four very excellent propositions, (but proved by no very easie Demonstrations)

The Preface.

ons) among divers others that have more of Geometrical Subtility, then usefulness, Those Mathematicians, that, (like Marinus Ghetaldus, Stevinus, and Galileo) have added anything considerable to the Hydrostaticks have been (that I know of) very few, and those too, have been wont to handle them, rather as Geometricians, then as Philosophers, and without referring them to the explication of the Phenomena of Nature. And as for the Peripateticks, and other School-Philosophers, though on some Occasions, as when they tell us, That water weighs not in water, nor aire in aire, they deliver assertions about matters belonging to the Hydrostaticks, (which term, in this Treatise, I often take in a large sense because most of the things delivered about the weight of Bodys may by easy variations, be made applicable to other Fluids) yet they are so far from having illustrated, or improved them, that they have but broach'd or credited

The Preface.

credited, divers of the most erroneous conceits, that are entertain'd about them. So that, there being but few Treatises written about the Hydrostaticks, and those commonly bound up among other Mathematical works, and so written, as to require Mathematical Readers, this usefull part of Philosophy, has been scarce known any farther then by name, to the generality ev'n of those Learned men, that have been inquisitive into the other parts of it, and are deservedly reckoned among the ingenious Cultivators of the modern Philosophy. But this is not all, For some eminent men, that have of late years, treated of matters Hydrostatical; having been prepossess'd with some erroneous Opinions of the peripatetick School, and finding it difficult, to consult experience, about the truth of their Conclusions, have interwoven divers erroneous Doctrines among the sounder propositions, which they either borrow'd from Archimedes, and other circumspect Mathematicians, or devis'd

The Preface.

vis'd themselves, and these mistakes being deliver'd in a Mathematical dress, and mingled with Propositions demonstrably true, the Reputation of such Learned Men, (from which I am far from desiring to detract,) and the unqualifiedness of most Readers, to examine Mathematical things, has procur'd so general an entertainment for those Errors, that now the Hydrostaticks is grown a part of Learning, which is not only difficult to attain, but dangerous to Study.

Wherefore, though neither the Occasion and designe of this Treatise exacted, nor my want of skill and leasure qualified me to Write either a Body or Elements of Hydrostaticks: yet I hop'd I might doe something, both towards the illustrating, and towards the rescue of so valuable a Discipline, by Publishing the ensuing Tract; where I endeavour to disprove the receiv'd errors, by establishing Paradoxes contrary to them, and to make the Truths
the

The Preface,

the better understood and receiv'd, partly by a way of Explicating them unemploy'd in Hydrostatical Books, and partly by confirming the things I deliver by Physical and sensible Experiments. And over and above this, the more to recommend Hydrostaticks Themselves to the Reader, I have, besides the Paradoxes, oppos'd to the Errors I would disprove, taken occasion by the same way, to make out some of the usefullest of those Hydrostatical Truths, that are wont to seem strange to Beginners.

If it be here demanded, why I have made some of my Explications so prelix, and have on several occasions inculcated some things. I answer, That those who are not us'd to read Mathematical Books, are wont to be so indispos'd to apprehend things, that must be explicated by Schemes, and I have found the generality of Learned men, and ev'n of those new Philosophers that are not skill'd in Mathematicks, so
much

The Preface.

much more unacquainted, then I before imagin'd both with the principles and Theorems of Hydrostaticks, and with the ways of explicating and proving them, that I fear'd, that neither the Paradoxes themselves, that I maintain, nor the Hypotheses about the weight and pressure of the aire, upon which, little less then my whole Pneumatical Book depends, would be thoroughly understood without such a clear Explication of some Hydrostatical Theorems, as to a person not vers'd in Mathematical writings, could scarce be satisfactorily deliver'd in few words. And therefore, though I do not doubt, that those who are good at the most compendious ways of demonstrating, will think, I might in divers places, have spar'd many words without injury to my proofs, and though I am my self, of the same mind I expect to find them of; yet, I confess that 'twas out of choice that I declin'd that close and concise way of writing, that in other cases I am want most

The Preface.

to esteem. For Writing now not to credit my self, but to instruct others, I had rather Geometricians should not commend the shortness of my Proofs, then that those other Readers, whom I chiefly design'd to gratifie, should not thoroughly apprehend the meaning of them.

But this is not all for which I am to excuse my selfe to Mathematicall Readers. For some of them, I fear, will not like that I should offer for Proofs such Physical Experiments, as do not alwayes demonstrate the things, they would evince, with a Mathematical certainty and accuratenesse; and much less will they approve, that I should annex such Experiments to confirm the Explications, as if Suppositions and Schemes, well reason'd on, were not sufficient to convince any rational man about matters Hydrostaticall.

The Preface.

In Answer to this I must represent, that in Physical Enquiries it is often sufficient that our determinations come very near the matter, though they fall short of a Mathematical Exactness. And I choose rather to presume upon the equity of the Reader, than to trouble him and my self with tedious Circumlocutions, to avoid the possibility of being misunderstood, or of needing his Candor. And we see, that even Mathematicians are wont, without finding any inconvenience thereby, to suppose all perpendicular Lines, made by pendulous Bodies, to be parallel to one another: Though indeed they are not; since, being produc'd, they would meet at the Centre of the Earth: And to presume, that the Surface of every calme water, in a Vessel, is parallel to the Horizon; and consequently, a Plain: Though, in strictness, themselves think it the portion of a Sphere: And though also I have usually

The Preface.

usually observ'd it to be higher, where 'tis almost contiguous to the sides of the Vessel, then 'tis in other places.

Moreover, since we find that though water will be uniformly rais'd in Pumps to several heights, but not to thirty five foot, and will in ordinary open pipes, be almost of the same level within and without, but not if the pipe be extraordinary slender; Upon these, and divers other such considerations, I may have sometimes made use of expressions, that seemed not positive and determinate enough to be employed about matters to which Mathematical Demonstrations are thought applicable. But I elsewhere give an account of the scruples I have about such Demonstrations, as they are wont to be apply'd to Physical matters. And, in the present Paradoxes, I think I have not done nothing, if in my Hydrostatical Explications I have made it appear, That in

The Preface.

Experiments made with such Liquors and Glasses, as I employed, the Rules will hold without any sensible, or at least any considerable Error ; for thereby we may learn the Truth of many things, for the main, though in some we should not have attained to the exactness of measures and proportions, which yet our endeavors may assist others to arrive at.

And as for my confirmation of Hydrostatical propositions by Physical Experiments, if some Readers dislike that way, I make no doubt but that the most will not only approve it, but thank me for it. For though, in pure Mathematicks, he that can demonstrate well, may be sure of the Truth of a Conclusion, without consulting Experience about it : Yet because demonstrations are wont to be built upon Suppositions or Postulates ; and some things, though not in Arithmetick or Geometry, yet in Physical matters, are wont

The Preface.

wont to be taken for granted, about which men are lyable to slip into mistakes ; even when we doubt not of the Ratiocination, we may doubt of the conclusion, because we may, of the Truth of some of the things it supposes. And this Consideration, if there were no other, will, I hope, excuse me to Mathematicians, for ventring to confute some reasonings that are given out for Mathematical demonstrations. For I suppose it will be consider'd, that those whose presum'd Demonstrations I examine, though they were some of them Professours of Mathematicks, yet did not Write meerly as Mathematicians, but partly as Naturalists : so that to question their Tenets, ought not to disparage those, as well certain, as excellent and most useful Sciences, pure Mathematicks, any more then that the Mathematicians that follow the Ptolemaick, the Copernican, the Tichonian, or other Systemes

The Preface.

if the world, Write Books to manifest one anothers Paralogismes in Astronomical matters: And therefore (to proceed to what I was about to say) it cannot but be a satisfaction to a wary man to consult sense about those things that fall under the Cognisance of it, and to examine by Experiences, whether men have not been mistaken in their Hypotheses and Reasonings, and therefore the Learned Stevinus himself (the chief of the Moderne Writers of Hydrostaticks) thought fit, after the end of his Hydrostatical Elements, to add in an Appendix some Pragmatical Examples (as he calls them) that is, Mechanical Experiments (how cogent I now inquire not) to confirm the Truth of his Tenth Proposition, to which he had, not far from the beginning of his Book, annexed what he thinks a Mathematical Demonstration. And, about the very Subjects we are now upon, the following Para-

The Preface.

Paradoxes will discover so many mistakes of eminent Writers, that pretend to have Mathematically demonstrated what they teach, that it cannot but make wary Naturalists (and 'tis chiefly to gratifie such that I publish this) be somewhat diffident of Conclusions, whose proofs they do not well understand. And it cannot but, to such, be of great satisfaction to find the things, that are taught them, verified by the visible testimony of Nature her self. The importance of this Subject, and the frequent Occasion I have to make use of this kind of Apology, will I hope, procure me the Readers pardon if I have insisted somewhat long upon it.

After what has been hitherto discours'd, 'twil be easie for me to give an Account, why I premised these Hydrostatical Paradoxes to the rest of the Appendix, wherewith they are * now publish'd: For since a great part of my work in that Appendix, was to be a further Explication

* An Account of this passage also, may be had from the Publishers Advertisement to the Reader.

The Preface.

of some things delivered in the Book it is
subjoyn'd to, and the vindication of them
from invalid objections: And since I have
generally observ'd, that the objections that
have bin, either publickly or privately,
made against the explications & reasonings
contain'd in that Book, were wont to proceed
from unacquaintedness, either with the true
notion of the weight and spring of the aire, as
I maintain them, or with the Principles and
Theorems of Hydrostaticks, or else from er-
roneous Conceits about them; I thought it
would much conduce to both the forementio-
ned ends of my Appendix, If I clear'd up
that Doctrine to which my Experiments and
reasonings have been all along Consonant,
& whose being either not known, or misun-
derstood, seems to have occasion'd the obje-
ctions that have been hitherto made against
the Hypotheses I have propos'd, or the
Explications I have thence given. And
however, since the Proofs I offer for my
opinions are for the most part drawn from
Expe-

The Preface.

Experiments new & easie, and that my aim is but to discover Truths, or make them out by clearer explications, without supposing, like those I dissent from, any thing that is either precarious or scarce, if at all, intelligible; I hope, that if I should not prove happy enough to reach my ends, yet the Ingenious and Equitable Reader will approve my Designe, and be advantaged by my Experiments. Of which some of the chiefest, and some of the most difficult, having been seen (divers of them more then once) by the Royal Society it self, or by inquisitive Members of it; it will, I presume, be but a reasonable request, if the Reader, that shall have the curiosity to try them over again, be desired not to be hasty in distrusting the matters of fact, in case he should not be able at first to make every thing succeed according to expectation. For as easie as I have endeavour'd to make these Experiments, yet I dare not promise myself that they will all of them be priviledg'd from the fate
where-

The Preface.

wherein I have observ'd other Physico-Mathematical ones to be not seldome obnoxious from some unheeded Physical Circumstance, by which those that are not acquainted with the subtleties of Nature, or, at least for the time, do not sufficiently consider them, are apt to be imposed upon.

This Advertisement will perhaps be best illustrated, & recommended by an instance. And therefore I shall subjoyne one that will possibly seem somewhat odd.

It has been taken notice of by two or three Ingenious modern Mathematicians, and I have had occasion to make it out by particular Experiments, that warm water is lighter in specie then cold: whence it has been deduc'd, that wax, and other Bodies, very near equiponderant with common water, will swim in that which is cold, and sink in that which is hot, or luke-warm. Which Experiment, though as it may be (and perhaps it has been) tryed, I readily allow to be agreeable to the known Laws of the Hydrostaticks;

Tet

The Preface.

Yet I have sometimes undertaken that the
Troyal should have a quite contrary event.
To this purpose having taken some yellow
Bees-wax, which was formed into a Pellet
of the bigness of a Cherry, and, by the help
of a little Lead, was made so near equipon-
derant to cold water, that, being but a very
little heavier, a very small diminution of
its weight would make it emerge, I remov'd
it out of the very cold water, into some that
had bin purposely made lukewarm, (or a
little more then so) where it quickly, some-
what to the wonder of the lookers on, appear'd
to swim on the top of the water. And that it
might not be suspected that it was suppor-
ted by any visible bubbles, which I have ob-
served, in some cases, to buoy up even heavy
Bodies, and deceive the unskilful, or un-
attentive; I briskly enough duck'd the bul-
let 2 or 3 times under water to throw them
off, notwithstanding which it constantly re-
turn'd to float, and yet being remov'd again
into the same cold water it had been taken
out

The Preface.

out of, and duck'd as before to free it from adherent bubbles, it lay quietly at the bottom, and, though rais'd several times to the upper part of the water, would immediately subside again, and fall to the very lowest. Now that w^{ch} invited me to promise an Experiment which seems to contradict the principles of the Hydrostaticks, was not any distrust of those principles themselves, but a conjecture, that as by warmth the water would be made a little lighter in specie then 'twas before; so by the same warmth the spirituous and more agitable parts of the wax, whose texture is loose enough, would be somewhat (though not visibly) expanded, and would by that expansion gain a greater advantage towards floating, then the increas'd lightness of the water would give it disposition to sink. And I confirm'd this conjecture by a farther experiment, which at first was it self somewhat surprising to the Beholders. For when the wax was first taken out of the cold water, & immediately immers'd in the warm,

The Preface.

warm, it would readily enough sink, & being (with a quill or a knife) rais'd to the top of the water, it would again fall down, but more slowly then at the begining, & after some few minutes, if it were rais'd to the upper parts of the water, it would remain a float. (And I have known it, when it had remain'd a while longer at the bottom, so to emerge, that if I were sure no unheeded bubbles had been newly generated, and held it up, it might be said to emerge of its own accord) as on the other side, being put into the cold water as soon as ever it was taken out of the warm, it would at the very first float, and being then knock'd downwards, it would, readily enough, regain the upper part of the water, but if I continu'd to send it downwards about 6 or 7 times (more or fewer) successively, it would emerge every time more slowly then other, and at length not emerge at all, even when I try'd it in water made heavy, by being highly infrigidated with salt and snow plac'd about the Glass. Which Phenomena I had
thought

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thought it reasonable to expect, because I presum'd, that the Wax being remov'd immediately out of the warm water, into the cold, must require some time, to loose the adventitious expansion, which the warmth had given it, and must be depriv'd of it by degrees, by the coldness of the water into which the wax was transferr'd. As on the other side, there must be some time necessary for so little a warmth, as that of the tepid (or little more then tepid) water, to give the wax that addition of dimensions (which also it must receive by degrees) that was necessary, in spite of the rarefaction of the water, to make it float. I might add, that these Tryals were repeated, for the main, with more Bullets of wax then one, and that they succeeded far otherwise, when, instead of a piece of wax, we imploy'd a pois'd glass bubble, in which the temperature could make either no change at all, or no considerable change of dimensions. And to these I might add other circumstances, if I did not remember,

The Preface.

ber, that I mention these Tryals but occasionally, and to make the caution, formerly recommended to the Reader, appear not to be impertinent, since a Hydrostatical Experiment, true in its self, may easily miscarry by over-looking such Circumstances as 'tis not easie to be aware of.

But by this Advertisement I would by no means divert Men from being diffident of Hydrostatical Traditions and Experiments. For, besides the many Erroneous Opinions, there are matters of fact, whose Truth, tho' not question'd, but built upon, I think ought to be brought to tryal. For, even whilst I was concluding this Preface, I found that divers even of the Moderns, & particularly a very learned Man that has lately Written of Hydrostaticks, have much troubled themselves to render a reason why, since, according to their Doctrine, water weighs not in water, Wooden vessels, though of a substance lighter then water, being by leaks, or otherwise, fill'd with water, should sink and remain at
the

The Preface.

the bottom of the water: whereas judging this Phenomenon disagreeable to what I look upon as the Laws of the Hydrostaticks, I was confirm'd in that opinion, by having had the curiosity to make some tryals of it, with 4 or 5 vessels of differing shapes and sizes, whereof two were of wax, which, though a matter but very little lighter then water, I could not sunck, or keep sunck by pouring water into them, or suffering them to fill themselves at leaks made near the bottom, and if they were depressed by force or weights, they, as also the wooden Vessels, would upon the removal of the impediment (and sometimes with the cavity upwards) emerge. And I am the more solicitous to have things in the Hydrostaticks duly ascertain'd, because the weighing of bodies in Liquors may hereafter appear to be one of the general ways I have employ'd, and would recommend, for the examining of almost all sorts of tangible Bodies.

HYDRO-

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- And so likewise,
- If the water that leans upon the Body be contain'd in pipes open at both ends; the pressure of the water is to be estimated by the weight of a pillar of water, whose Basis is equal to the lower Orifice of the pipe, (which we suppose to be parallel to the Horizon) and its height equal

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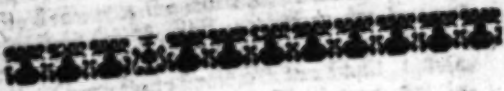
HYDRO.

Imprimatur

ROBERTUS, S.A.D.


VICE-CANCELLARIUS

O X O N



Imprimatur,

ROBERTUS SAT,
VICE-CANCELLARIUS
OXON.



(1)



HYDROSTATICAL PARADOXES,

Made out by
NEW EXPERIMENTS:

Presented to the
ROYAL SOCIETY;
(*The Lord Viscount Brouncker being
then President.*) May 1664.

My LORD,



O obey the orders of the
Society, that forbid the
making of Prefaces and
Apologies in Accounts
of the Nature of that
which you expect from
me; I shall without any further pre-
amble begin with taking notice, that
B upon

upon perusal of *Monsieur Paschall's* small French Book, which was put into my hands, I find it to consist of two distinct Treatises: The one of the *Æquilibrium* of Liquors, as he calls it; and the other of the weight of the *Mass* of the *Air*.

As for this latter, (which I shall mention first, because I can in very few words dispatch the little I have to say of it) Though it be an ingenious discourse, and contains things, which if they had been published at the time, when it is said to have been written, would probably have been very well-come to the Curious: yet I have very little else to say of it in this place, in regard that since that time, such kind of Experiments have been so prosecuted, that I presume it is needless, and would not be acceptable to repeat what *Monsieur Paschall* has written, in this Society, which has seen the same

Truths

Truths, and divers others of the like Nature, more clearly made out by Experiments, which could not be made by *Monsieur Paschall*, and those other Learned Men, that wanted the advantage of such Engines and Instruments, as have in this place been frequently made use of.

Wherefore having already at a former meeting given you, by word of Mouth, an account of *Monsieur Paschall's* Ingenious Invention, of a pair of Bellows without vent, to measure the various Pressure of the Atmosphere; I remember nothing else that needs hinder me from proceeding to the other part of his Book, *The Treatise of the Equilibrium of Liquors*.

This I find so short, and so worthy of the Author, that to give you all that I judge worth taking notice of in it, would oblige me to transcribe almost the whole Tract; and therefore I shall

rather invite you to read the whole, then divert you from the designe by culling out any part of it; yet if you will not be satisfied without something of more particular, I shall be oblig'd to tell you, That the Discourse consisting partly of Conclusions and partly of Experiments; the former seemed to me to be almost all of them (there being but few that I doubt of) consonant to the Principles and Lawes of the **Hydrostaticks**. But as for the latter, the Experimental proofs he offers of his opinions are such, that I confess I have no mind to make use of them.

And the Reasons why, notwithstanding that I like most of *Monsieur Paschall's* Assertions, I decline imploying his way of proving them, are principally these.

First, Because though the Experiments he mentions be delivered in such a manner, as is usual in mentioning matters

matters of fact; yet I remember not that he expressly says that he actually try'd them, and therefore he might possibly have set them down as things that *must* happen, upon a just confidence that he was not mistaken in his Ratiocinations. And of the reasonableness of this Doubt of mine, I shall ere long have occasion to give an instance.

Secondly, Whether or no *Monsieur Paschall* ever made these Experiments himself; he does not seem to have been very desirous, that others should make them after him. For he supposes the Phenomena he builds upon to be produc'd fifteen or twenty foot under water. And one of them requires, that a Man should sit there with the End of a Tube leaning upon his Thigh. But he neither teaches us how a Man shall be enabled to continue under water, nor how in a great Cistern full of

water, twenty foot deep, the Experimenter shall be able to discern the alterations, that happen to *Mercury* and other Bodies at the Bottome.

And *Thirdly*, These Experiments require not only Tubes twenty foot long, and a great Vessel of at least as many feet in depth, which will not in this Countrey be easily procured, but they require Brass Cylinders, or Pluggs, made with an exactness, that, though easily supposed by a Mathematician, will scarce be found obtainable from a Tradesman.

These difficulties making the Experiments propos'd by *Monsieur Paschall* more ingenious then practicable, I was induc'd on this occasion to bethink my self, of a far more Expeditious Way, to make out, not only most of the Conclusions wherein we agree, but others that he mentions not; and this with so much more ease and clearnesse,
That

That not only This Illustrious Assembly, but persons no more than moderately vers'd in the Vulgar principles of the Hydrostaticks, may easily enough apprehend what is design'd to be deliver'd, if they will but bring with them a due Attention, and minds dispos'd to preferre Reason and Experience to vulgar Opinions and Authors; which last clause I annex, because the following Discourse, pretending to confute several of those, challenges a right to except against their Authority.

It not being my present Task to deliver the Elements, or a Body of Hydrostaticks, but only ten or twelve Paradoxes, which I conceive to be provable by this new way of making them out, I shall, to avoid Confusion, Deliver Them in as many distinct propositions; After each of which, I shall endeavour in a proof, or an Explication,

plication, to show, both that it is true, and why it ought to be so. To all these I shall to avoid needless Repetitions, premise a word or two by way either of *postulatum* or *Lemma*.

And because I remember to what Assembly I address This Discourse, I shall make use of no other then an easie supposition I met with in a short Paper (about a Mercuriall Phenomenon) brought in a year or two since to this Learned Society, by a deservedly Famous Member of

* That excellent Mathematician the Learned Dr Wallis, Savilian Professor of Geometry,

it*, For though his supposal be made upon occasion of an Experiment of ano-

ther Nature, then any of the ensuing, it may be easily accomodated to my present purpose.

This *postulatum* or *Lemma*, consists of three parts; the first of them more, and the two last, less principal.

Suppose

Suppose we then, (*First*) That if a Pipe open at both Ends, and held perpendicular to the Horizon, have the lower of them under Water, there passes an Imaginary plain or Surface, which touching that Orifice is parallel to the Horizon; and consequently parallel as to sense to the upper Surface of the water, and this being but a help to the Imagination will readily be granted.

Secondly, To this it will be consonant, that each part of this designable surface, will be as much, and no more press'd, as any other equal part of it, by the water that is perpendicularly incumbent on it. For the water or other Fluid being supposed to be of an homogeneous substance, as to gravity, and being of an equal height upon all the parts of the imaginary Surface; there is no reason why one part should be more press'd by a perpendicular pillar of that incumbent fluid, than any other

other equal part of the same Surface by another perpendicularly incumbent pillar of the same or equal Basis and height, as well as of the same Liquor.

But *Thirdly*, Though whilst our imaginary Surface is equally press'd upon in all parts of it, the Liquor must retain its former position; yet if any one part comes to have a greater weight incumbent on it, then there is upon the rest, that part must be displac'd, or depress'd, as it happens, when a stone or other Body heavier then water sinks in water. For wherever such a Body happens to be underneath the water, that part of the imaginary plain that is contiguous to the lower part of the stone, having on it a greater weight then other parts of the same Surface, must needs give way, and this will be done successively till the stone arrive at the Bottom; and if, on the other side, any part of the Imaginary Surface be

less

less press'd upon then all the rest; it will by the greater pressure on the other parts of the Surface be impell'd upwards, till it have attain'd a height, at which the pressure (of the rais'd water, and the lighter or floating Body (if any there be) that leans upon it, and gravitates together with it, upon the subjacent part of the Imaginary Surface) will be equal to that which bears upon the other parts of the same Surface.

And because this seems to be the likeliest thing to be Question'd in our Assumption, though he

that considers it attentively, will easily enough be induc'd to grant it:

Yet I shall here endeavour to evince it Experimentally, and that

by no other way of proof, then the same I imploy all along this present discourse.

This Experiment and the Explication of it, if to some they should here seem somewhat obscure, will be easily understood by the Figures and Explications belonging to the first ensuing Paragraph.

Take

Take then a Cylindrical glass pipe, of a convenient Bore open at both Ends, let the Tube be steadily held perpendicular to the Horizon, the lower end of it being two or three inches beneath the Surface of a convenient quantity of water, which ought not to fill the Glass Vessel that contains it. The pipe being held in this posture, 'tis manifest, that the water within the pipe, will be *almost* in a level with the Surface of the water without the pipe, because the external and internal water (as I am wont for Brevities sake to call them) have free intercourse with one another by the open Orifice of the immers'd End of the pipe: yet I thought fit to insert the word *almost*, because if the pipe be any thing slender, the Surface of the water in it, will always be somewhat higher then that of the water without it, for reasons that 'tis not so necessary we should now inquire after,

after, as 'tis, that we should here desire to have this taken notice of once for all; That mistakes may be avoided without a troublesome repetition of the difference in heights of the Surface of Liquors within pipes and without them, in case they be any thing slender.

The pipe being held in the newly mention'd posture, if you gently poure a convenient Quantity of Oyle upon the external water, you shall see, That as the Oyle grows higher and higher above the Surface of That water, the water within it, will rise higher and higher, and continue to do so, as long as you continue to poure on oyle; Of which the Reason seems manifestly to be this; That in the Imaginary plaine that passes by the Orifice of the immers'd end of the pipe, all that is not within the Compass of the Orifice, is expos'd to an additional pressure from the

the weight of the oyle which swims upon the water, and that pressure must still be increas'd, as there is more and more oyle poured on; whereas a Circular part of the Imaginary plain, equal to the Orifice of the Glasse, is by the sides of the pipe fenc'd from the immediate pressure of the oyle; so that all those other parts of the water, being far more press'd, then that part which is comprehended within the Cavity of the Tube: and consequently the press'd parts of the external water, are by the equal gravitation of the oyle, upon the parts of the external water, impell'd up into the Cavity of the pipe, where they find less resistance, then any where else, till they arrive at such a height, that the Cylinder of water, within the pipe, do's as much gravitate upon the subjacent part of the Imaginary Surface, as the water and oyle together, do upon every other equal

equal part of the same Surface or plain.

But as well the former *Lemma*, as this Experiment, will be sufficiently both clear'd and confirm'd by the following Explications; to which I should for that Reason forthwith proceed; Were it not that, since divers passages of the following Treatise suppose the Aire to be a Body not devoid of weight, which yet divers Learned adherents to the Peripatetick Philosophy do resolutely deny, it seems requisite to premise something for the proof of this Truth.

And though I think the Arguments we have imploy'd to that purpose already, do strongly evince it: yet if I may be allow'd to anticipate one of my own Experiments of the Appendix, I shall give an instance of the weight of the Aire, not lyable so much as to those invalid objections, which some of the Aristotelians have made against those
Proofs

Proofs, wherewith we have been so happy, as to satisfie the learned & even of our professed Adversaries.

We caus'd then to be blown at the flame of a Lamp, a Bubble of glass, (of about the bigness of a small Hen-egge) which, that it might be light enough to be weigh'd in exact Scales, ought to be of no greater thickness, then is judg'd necessary to keep it from being (when seal'd up with none but very much expanded aire in it) broken by the pressure of the ambient Atmosphere. This bubble was (like a Peare with its stemme) furnish'd with a very slender pipe of Glass, at which it was blown, that it might be readily seal'd up; and then (the Aire within it being by the flame of the Lamp gradually rarified, as much as conveniently could be) whilst the Body of the Bubble was exceeding hot, the newly mentioned stemme was nimble put into the

the middle of the flame; where, by reason of its slenderness, the Glass, which was exceeding thin, was immediately melted; whereby the Bubble was Hermetically seal'd up. This Glass being permitted leisurely to coole, I could afterwards keep it by me an hour, or a day, or a week, or longer, if I thought fit; and when I had a mind to shew the Experiment, I put it in one of the scales of an exact ballance, that would turn, perhaps with the 30th, or 50th, or a lesse part of a grain; and having carefully counterpois'd it, I then warily broke off the seal'd end, placing a sheet of paper just under the scale to receive the fragments of the Glass: and putting in again those fragments, that scale wherein the Glass was would considerably preponderate; which it must do upon the account of the Weight of Aire, there being no other cause, either
C
needful,

needful, or justly assignable, but the weight of the Aire that rush'd into the Cavity of the Glass, as finding less resistance there then elsewhere, by reason that the included Aire had it's spring much weakn'd by it's great expansion.

This Experiment I many times tryed, sometimes before some *Virtuosi*, and sometimes before others; who all allowed it to be conclusive. For here it could not be objected as against the weighing of Aire in a Bladder, (which objections yet I could easily answer, if it were *now* proper) that the aire which ponderates, is stuff'd with the Effluvia of him that blows the Bladder, and (besides that) is not aire in it's Natural state, but violently compress'd. For here 'tis the free aire, and in it's wonted laxity, that makes the Glass preponderate.

And that there is a great Ingress of
the

the external aire, is evident by these three Phænomena. The *one*, that if you lend an attentive Ear, you shall plainly heare a kind of whistling noise to be made by the external aire, as it rushes violently in upon the breaking of the Glass; The *other*, that the Rarefaction of the aire, seal'd up in the bubble, being very great, there is a great deal of space left for the ambient aire to fill upon its admission; and the greatness of this Rarefaction may be guess'd at, both by the breaking of such bubbles now and then by the pressure of the External aire, which is not competently assisted by the Internal to resist; and also by the *third* Phænomenon I intended to take notice of, namely, That if, instead of breaking off the seal'd end of the Glass in the aire, you break it under water, that Liquor will, by the Pressure of the Atmosphære, be forc'd to spring up like an artificial Fountaine

into the Cavity of the Bubble, and fill about three quarters of it. By which last circumstance I gather, that the weight of the aire is more considerable then ev'n many, who admit the aire to have weight, seem to imagine. For we must not suppose, that all the aire contain'd in the Bubble, when broken, weighs no more then the weight requisite in the opposite Scale, to reduce the Ballance to an *Æquilibrium*; since this additional weight is onely that of the aire, that intrudes on the breaking of the glass; which aire, by the Observations newly mention'd to have been made with water, appears to be but about three quarters of the whole aire contain'd in the broken Bubble; and yet, according both to our Estimate, and that of divers Virtuosi, and some of them eminent Mathematicians, when the capacity of the Bubble was short of two cubical Inches, (and so proportionably

tionably in other glasses,) the nice Balance we us'd, manifested the newly admitted Aire to amount to some times near halfe a grain, and sometimes beyond it.

And because one of the last Experiments that I made to this purpose, with seal'd Bubbles was none of the least accurate, I shall conclude this Subject with the following account of it.

A thin glass Bubble, blown at the flame of a Lamp, and Hermetically seal'd when the contained aire was exceedingly rarified, was Counterpoiz'd in a nice paire of Scales, and then the seal'd *apex* being broken off, and put again into the same Scale, the weight appear'd to be increas'd by the re-admitted aire, a pretty deal above $11\frac{1}{2}$, and consequently very near, if not full $\frac{1}{2}$ of a graine: Lastly, having by some slight (for 'tis no very easie matter) fill'd it

with common water, we weigh'd the glass and water together, and found the latter, besides the former, to amount to 906 grains: so that supposing, according to our former Estimate, countenanced by some Tryals, that the re-admitted aire, which amounted to $\frac{1}{4}$ of a grain, fill'd but $\frac{1}{4}$ of the whole Cavity of the Bubble, the aire that was in it, when seal'd, possessing one quarter of that Cavity, the whole aire contain'd in the Bubble, may be reasonably presum'd to weigh a whole grain; in which case we might conclude (abstracting from some little Niceties not fit to be taken notice of here; as elsewhere) that the water in our Experiment, weighed very little more then nine hundred times as much as an equal quantity of Aire, And therefore, though we allow, that in an Experiment so diligently made, as this was, the aire præexistent in the bubble did not adæquately possess so much

as a fourth part, but about a fifth or a sixth of its Cavity, the aire will yet appear so heavy, that this Experiment will agree well with those others, recorded in another Treatise, wherein we assign'd (*numero rotundo*) a thousand to one, for the proportion wherein the specifick Gravity of water exceeds that of aire.

PARA-



PARADOX I.

That in Water, and other Fluids, the lower parts are press'd by the upper.

PROvide a Glass vessel of a convenient height and breadth A. B. C. D. fill'd with water almost to the Top; Then take a glass Pipe, open at both Ends, Cylindrical, and of a small Bore, (as about the eighth or sixth part of an Inch in Diameter.) Put the lower End of this Pipe into clear Oyle or Spirit of Turpentine; and having by Suction rais'd the Liquor to what part of the Pipe you think fit, as soon as it is there, you must, very nimbly removing your Lips, stop the upper Orifice with the pulp of your finger, that the rais'd Liquor may not fall back again: Then taking

taking the Pipe and that Liquor out of the Oyle of Turpentine, place it perpendicularly in the Glass of water, so as that the Surface of the Oyle in the Pipe be somewhat higher then that of the water without the Pipe; and having so done, though you take off your finger from the upper Orifice of the Pipe, the Oyle will not fall down at the lower Orifice, though that be open, but will remain suspended at the same height, or near thereabouts, that it rested at before.

Now Oyle of Turpentine, being a heavy Fluid, does, as such, tend downwards, and not being stopp'd by the Glass it self, whose lower Orifice is left open, it would certainly fall down through the Pipe, if it were not kept suspended by the pressure (upwards) of the water beneath it. There appearing no other Cause to which the Effect can reasonably be ascrib'd, and this being
 suffici-

sufficient to give an Account of it, as we shall presently see. For that it is not any contrariety in Nature, betwixt the oyle and the water, as Liquors that will not mingle, is evident from hence, That if you had remov'd your finger when the Pipe was not so deeply immers'd in the Glass, but that the Surface of the oyl in the Pipe was an Inch or two more elevated above that of the water in the Glass, then in our present case we suppose it to be; The Oyle, notwithstanding its presum'd contrariety to water, would have freely subsided in the Pipe, till it had attain'd an æquipollency of pressure with the External Water.

The Reason therefore of the Phænomenon seems to be plainly this. Supposing the imaginary surface, on which the Extremity Q of the pipe PQ leans, to be GH . If that part of the Surface, on which the Oyl leans at Q , be as much, and no more charged, or press'd upon
by

by the weight of the incumbent Cylinder of Oyle $\mathcal{Q}X$, then the other parts of the same imaginary Surface GH are by the water incumbent on Them, there is no Reason why that part at \mathcal{Q} should be displac'd, either by being depress'd by the weight of the Cylinder of Oyle $X\mathcal{Q}$, or rais'd by the equal pressure of water upon the other parts of the Superficies GH .

And that this *Æquilibrium*, betwixt the Oyle and the Water, is the true cause of the Phænomenon, may be confirm'd by observing what happens, if the altitude of either of the two Liquors be alter'd in Relation to the other.

And (*First,*) we have already taken notice, That if the Cylinder of Oyle reach in the Pipe, much higher then that of the Surface of the water, the oyle will descend: Of which the Reason is, Because the designable Surface GH , being more charg'd at \mathcal{Q} then
any

any where else, the part *Q*, being unable to resist so great a pressure, must necessarily be thrust out of place by the descending oyle.

Secondly, This subsiding will continue but till the Surface of the Oyle in the Pipe be fallen almost as low as that of the water without the Pipe; because then, and not before, the parts at *Q* are but as much press'd by the oyle, as the other parts of the Surface *G H* are by the water that leans upon them.

Thirdly, 'Tis a concluding Circumstance to our present purpose, That if the Oyle and Water being in an *Æquilibrium*, you gently lift up the Pipe, as from *Q* to *S*, the depth of the water being lessend, the oyle in the Pipe will grow præponderant, and therefore will fall out in Drops or Globuls, which by the greater Specifick Gravity of the water, will be buoy'd up to the Top of the Liquor, and there flote: And still

still as you lift up the Pipe higher and higher, towards the Surface $L M$, more and more of the Oyle will run out. But if you stop the Pipe any where in its Ascent, as at S , the Effluxion of the oyle will likewise be stopp'd. And at the imaginary Superficies $J K$, as by Reason of the shallowness of the water from L to J , or M to K , the pressure of the water upon the other parts of the Surface is not near so great, as it was upon the Surface $G H$, where the water had a greater depth : So by reason of the proportionate Effluxion of the oyle, whilst the Pipe was lifted up from Q to S , the remaining Cylinder of oyle incumbent on S , is not able to press that part of the Superficies $J K$ more strongly then the other parts of the same Superficies, are prest by the water Incumbent on them. And if the Pipe be lifted up till the lower Orifice be almost rais'd to V ; that is, almost

as high as the uppermost Surface of the water $L M$, so much of the oyle will, for the Reason already given, run out, that there will scarce be any left in the Pipe $T V$.

Fourthly, But if when the Pipe rests at the Surface $G H$, where the oyle is in an *Æquilibrium* with the water, you should instead of lifting it from Q to S , thrust it down from Q to O ; then the External water would not only sustaine the oyle, but make it ascend in the Pipe to a height equal to the distance $E G$; and so the Pipe will containe besides a longer Cylinder of oyle $Æ W$, a shorter one of water $Æ O$. For the pipe being transferr'd from the position $P Q$, to the position $O N$, there is a new Imaginary Surface $E F$, that passes by the lower Orifice of the Pipe. Now the part of this Surface at O will not, by the Incumbent oyle alone, be press'd as much as the other parts of the same

Sur-

Surface are by the Incumbent water. For the oyl alone was but in *Æquilibrium* with the water, when it was no deeper then LG , or HM ; so that the other parts of the Superficies EF , being more press'd upon by the water, then the part at O by the oyle, the oyle must give place, and be buoy'd up by the water, (which, if it were not for the weight of the oyle, would be impell'd up into the pipe full as high as the Surface of the External water) till the pressure of the admitted water OE , and the Cylinder of oyle EW , do both together gravitate as much upon the part O , as the rest of the Incumbent water does upon the other parts of the same Superficies EF .

Fifthly and lastly, 'Tis very agreeable to what has been delivered, touching the *Æquilibrium* of the oyle and water in the pipe PQ , that the Surface X of the oyle in the pipe, will not be

be of the same level with LM , that of the External water, but a little higher than it. For though the slenderness of the Pipe do somewhat contribute to this Effect, yet there would be an inequality, though not so great, betwixt these Surfaces upon this Account, That oyle of Turpentine being in *Specie*, (as they speak in the Schools) that is bulk for bulk, a lighter Liquor than Water, it is requisite that the height of it, incumbent on the part Q , be greater than that of the water on the other parts of the same Surface GH , to make the pressure of the oyle on the part it leans upon, equal to the pressure of the water on the other parts of the Surface. And if the inequality were greater betwixt the Specifick Gravities of these two Liquors, the inequalities betwixt the Surface X , and the Surface LM would be also greater, as may be try'd by substituting for com-

mon

mon water, oyle of Tartar *per deliqui-*
um, which is a saline Liquor much
 heavier than it. And that, in case the
 Pipe containe not a lighter Liquor
 then the External fluid, the Surface of
 the Liquor in the Pipe will not be high-
 er than that of the Liquor without it,
 we shall by and by have opportunity to
 manifest by Experience.

From what has been hitherto shewen,
 we may safely infer the Proposition, up-
 on whose occasion all this has been de-
 livered. For since the oyle in a Pipe,
 open at both Ends, may be kept sus-
 pended in any part under water, as at *Q*,
 because it is there in an *Equilibrium*
 with the External water; and since being
 lifted up in the water, as from *Q* to *S*,
 the oyle can no longer be kept suspen-
 ded, but by its own gravity will runne
 out. And since, in a word, the deep-
 er the water is, the greater weight and
 pressure is requir'd in the Cylinder of

D
oyle,

oyle, to be able to countervail the pressure of the water, and keep it self from being lifted up thereby; there seems no cause to doubt but that the parts of the water incumbent on the Superficies *G H*, do more press that Superficies, than the parts of the water contiguous to the Superficies *J K* do press that; and consequently, that the parts of the water that are under the uppermost Surface of it, are press'd by those of the same Fluid that are directly over them: As we saw also that the upper parts of the oyle, whilst the pipe was in raising from *Q* to *S*, depress'd the lower so much, as to force them quite out of the Pipe; there being in these cases no reason why the lowermost parts of a Liquor should press more, or have a stronger Endeavour against any other Liquor (or any other Body) the higher the Liquor incumbent reaches, if these inferior parts deriv'd their pressure only

ly from their own particular Gravity, (which is no greater then that of the other Homogeneous parts of the Liquor) and therefore they must derive the great force wherewith they press from the weight of the Incumbent parts, which consequently must be allow'd to press upon them.

But before I proceed to the following propositions, it will not be amiss to mention here, once for all, a few advertisements, to avoid the necessity of repeating the same things in the sequel of the Discourse.

And *First*, What is here said of the pressure of the parts of water upon one another, and the other Affections that we shall attribute to it, in the following paper, are to be apply'd to heavy Fluids in general, unless there shall appear some particular Cause of excepting some of them in particular Cases.

Secondly, Whereas I lately intima-

ced, That the inequality betwixt the Surfaces of the oyle in the Pipe, and of the External water, was in part to be ascrib'd to the slenderness of the Pipe, to be employ'd in these Experiments, I did it for this cause, that, whatever the Reason of it be, (which we need not here inquire after,) we are assur'd by Experience, as we have elsewhere shewn, That when Glass pipes come to be slender, water and many other Liquors (though not Quicksilver) will have within them a higher Surface then that of the same Liquor without them, and this inequality of Surfaces (as far as we have yet try'd) increases with the slenderness of the pipe. But this, as to our present Experiment, is a matter of so little moment, That it may suffice to have intimated that we did not oversee it.

Thirdly, Wherefore, notwithstanding this little inconvenience of slender Glasses,

Glasses, we think it Expedient to im-
 ploy such in the following Experi-
 ments, because we found, that in those
 of a wide Bore, upon such little ine-
 qualities of pressure as are not easily to
 be avoided, the oyle and water will pass
 by one another in the Cavity of the
 pipe, and so spoile the Experiment,
 which requires that the oyle within the
 pipe be kept in an intire and distinct
 Body.

Fourthly, Common oyle and water,
 or any other two Liquors that will not
 mingle, may serve the turn in most of
 these Experiments; but we rather chuse
 oyle of Turpentine, because it is light
 and thin, clear and colourless, and may
 be easily had in quantities, and is not
 so apt to spot ones Cloaths, or obsti-
 nately to adhere to the porous Bodies it
 chances to fall on, as Common, and o-
 ther express'd oyles. And for their
 sakes to whom the odour is offensive,

we presently correct it, by mingling with it a convenient quantity of oyle of Rhodium, or some other Chymical oyle that is odoriferous.

Fifthly, Oyle of Turpertine, though it be not reckon'd among the saline Menstruums, will yet (as we elsewhere note) work upon Copper, and so by digesting it upon crude filings of that Metal, we obtaine a deep green Liquor, which may be made use of instead of the Limpid oyle, to make the Distinction of the Liquors more conspicuous.

Sixthly, And for the same purpose we often use instead of clear water, a strong Decoction of Brazill, or Loggwood, or else Red Inck it self. I say, a *strong Decoction*, because unless the Liquor be so deeply ting'd, as to appear Opacous in the Glass, when it comes into the slender pipe, its Colour will be so diluted, as to be scarce discernable.

Seventhly,

Seventhly, In the shape of the Glass Vessel, we need not be Curious; though that of a wide Mouth'd Jarr, express'd in the Scheme, be for some uses more convenient than other shapes. The depth of these Glasses, and the length of the Pipes must be determin'd by the Experiments, about which one means to imploy them. To make out the first Paradox already prov'd, a Glass of about five or six Inches deep, and a Pipe about as many Inches long, will serve the turn: but for some others of the following Experiments, tall Cylindrical Glasses will be requisite; and for some, Broad ones likewise will be Expedient.

Eighthly, One must not be discourag'd by not being able at the first or second time, to suck up oyle of Turpentine to the due height, and stop it with ones finger from relapsing; but one must try again, and again; especi-

ally since many Tryals of this kind may be made in a few Minutes : and for Beginners 'tis a safe and good, though not the shortest way, to suck up rather more Liquor then one judges will be needful ; because having fill'd the Pipe to that height, you may by letting in the Airé warily and slowly, between the Orifice of the Glass and the pulp of your finger, suffer so much Liquor to run out of the Pipe, as will reduce it to the height you desire ; and there, by close stopping the Orifice with your finger, you may keep it suspended as long as you please, and immerse it into any Heterogeneous Liquor, and take it out again at pleasure without spilling any of it. By which slight Expedient alone, I can decline several Difficulties, and do many things, which, according to *Monsieur Paschal's* way, require a great deal of Trouble and Apparatus to be perform'd.

Lastly,

Lastly, In such Experiments where
 it may be of use, That there be a con-
 siderable disparity betwixt the two un-
 mingled Liquors, we may (as is above
 intimated) instead of fair water, imploy
Oleum Tartari per deliquium, and tinge
 it with Brazill or Chochineeale; from
 either of which, but especially from
 the latter, it will obtaine an exceeding
 deep Redness: and where one would
 avoid strong sentts and oylineess, he may,
 if he will be at the Charge, imploy
 oyle of Tartar *per deliquium*, instead of
 fair water, and highly Rectified Spirit
 of Wine, instead of oyle of Turpentine.
 For these two Liquors, though they will
 both readily mingle with water, will
 not with one another; and if a great
 quantity of some other Liquor be to
 be substituted for simple water, when
 these Chymical Liquors are not to be
 had in plenty, one may imploy (as we
 have done) a very strong Solution made
 of

of Sea-salt, and filtred through Cap-
 paper: this Brine being near about as
 Limpid as common water, and farre
 heavier than it. And for a Curiosity,
 we have added to the two lately men-
 tioned Liquors (oyle of Tartar, and
 Spirit of Wine) some oyl of Turpentine,
 and thereby had three Liquors of diffe-
 rent Gravities, which will not by shak-
 ing, be brought so to *minge*, as not quick-
 ly to part again, & retire each within its
 own Surface; and by thrusting a Pipe
 with water in the bottom of it (placing
 also ones finger upon the upper Orifice)
 beneath the Surface of the lowermost
 of these Liquors, and by opportunely
 raising or depressing it, one may some-
 what vary the Experiment, in a way not
 unpleasant; but explicable upon the
 same grounds with the rest of the Phae-
 nomena mentioned in this Discourse.

PARADOX I I.

That a lighter Fluid may gravitate, or weigh upon a heavier.

I Know that this is contrary to the common opinion, not only of the Schools; but ev'n of divers hodiern Mathematicians, and Writers of Hydrostaticks; some of whom have absolutely rejected this Paradox, though they do but doubt of the truth of the former.

But when I consider, that whether the cause of Gravity be the pulsion of any superior substance, or the Magnetical attraction of the Earth, or whatever else it be, there is in all heavy Bodies, as such, a constant tendency towards the Centre, or lowermost parts of the Earth; I do not see why that tendency

tendency or endeavour should be destroyed by the interposition of any other heavy Body; Though what would otherwise be the effect of that endeavour, namely an approach towards the Centre, may be hindered by another Body, which being heavier then it, obtains by its greater gravity a lower place; but then the lighter Body tending downwards, must needs press upon the heavier that stands in its way, and must together with that heavier press upon whatever Body it is that supports them both, with a weight consisting of the united gravities of the more, and the less heavy Body.

But that which keeps Learned Men from acknowledging this Truth, seems to be this, That a lighter Liquor (or other Body) being environ'd with a heavier, will not fall down but emerge to the Top; whence they conclude, that, in such Cases, it is not to be considered

sidered as a heavy, but as a Light Body.

But to this I answer, That though in Respect of the heavier Liquor, the less heavy may in some sence be said to be light; yet, notwithstanding that relative or Comparative Levity; it retains all its absolute Gravity, tending downwards as strongly as before; though by a contrary and more potent Endeavour upwards of the contiguous liquor (whose lower parts, if less resisted, are pressed upwards by the higher elsewhere incumbent; according to the Doctrine partly delivered already, and partly to be cleared by the proof of the next proposition,) its endeavor downward is so surmounted that it is forcibly carry'd up. Thus when a piece of some light wood being held under water, is let go and suffer'd to emerge, though it be buoy'd up by the water, whose specifick Gravity is greater, yet ev'n whilst it ascends it remains a heavy Body;

Body ; so that the aggregate of the water & the ascending wood weighs more then the water alone would doe ; And when it floats upon the upper part of the water, as part of it is extant above the surface , so part of it is immerst beneath it , which confirms what we were saying, That a lighter Body may gravitate upon a heavier.

And thus there is little doubt to be made but that if a man stand in one of the scales of a Ballance with a heavy stone ty'd to his hand , and hanging freely by his side , if then he lift that weight as high above his head as he can , notwithstanding that the stones motion upwards makes it seem a light Body in respect of the Man whose Body it leaves beneath it , yet it does not, either during its ascent or after, loose any thing of its connatural weight. For the Man that lifts it up shall feel its tendency downwards to continue, though

though his force, being greater than that tendency, be able, notwithstanding that tendency, to carry it up: and when it is aloft, it will so press against his hand, as to offend, if not also to bruise it; and the Stone, and the Man that supports it, will weigh no less in the Scale he stands in, then if he did not at all support it, and they were both of them weigh'd apart.

Likewise, if you put into one Scale a wide mouth'd Glass full of water, and a good quantity of powder'd common Salt; and into the other Scale, a Counterpoise to them both; you may observe, that, though at the beginning the Salt will manifestly lie at the bottom, and afterwards by degrees be so taken up into the Body of the Liquor, that not a grain will appear there; yet nevertheless (as far as I can judge by my Experiments) the weight in that Scale will not be diminished by the weight

weight of as much Salt as is incessantly either carried up, or supported by the restless motion of the dissolving Corpuscles of the water; but both the one and the other, (allowing for what may evaporate) will concur to gravitate upon the Scale that the glass containing them leans on.

But of this more elsewhere. Now to prove the proposition by the New Method, we have propos'd to our self in this Discourse.

Take a slender Glass pipe, and having suck'd up into it fair water, to the height of 3 or 4 inches, stop nimblely the upper Orifice with your finger, and immerse the lower into a Glass full of oyle of Turpentine, till the Surface of the oyle in the Vessel be somewhat higher than that of the water in the Pipe; then removing your finger, though the Pipe do thereby become open at both Ends, the water will not fall

fall down, being hinder'd by the pressure of the oyle of Turpentine. As will be obvious to them that have attentively consider'd the Explication of the former Paradox; there being but this difference between this Experiment and that there Explain'd, that here the water is in the Pipe, and the oyle in the Vessel, whereas there the oyle was in the Pipe, and the water in the Vessel. And if you either poure more oyle into the Glass, or thrust the Pipe deeper into the oyle, you shall see that the water will be buoyed up towards the top of the Pipe; that is, a heavier Liquor will be lifted up by a lighter. And since, by the Explication of the first Proposition, it appears, that the Reason why the Liquor is in this case rais'd in the Pipe, is the Gravity of the Liquor that raises it, we must allow that a lighter Liquor in *specie*, may by its gravity press against a heavier.

E

And

And it agrees very well with our Explication, both of this, and of the first Experiment; that as there, the Surface of the oyle in the pipe was always higher than that of the water without it, because the oyle being the lighter Liquor, a greater height of it was required to make an *Equilibrium*; so in our present Experiment, the Surface of the Liquor in the Pipe will always be lower than that of the oyle without it.

See the second Figure. For in the imaginary plain *E F*, the Cylinder of water *J G*, contain'd in the Pipe *J H*, will, by reason of its greater gravity, press as much upon the part *J*, as the distill'd oyle (*K E*, *J L*) being a lighter Liquor, can do upon the other parts of the same suppos'd plain *E F*, though the oyle reach'd to a greater height above it.

This second Paradox, we have hitherto been discoursing of, may be al-

to provid by what we formerly deliver'd, to make out the Truth of the third part of the Lemma premised to these Propositions.

But because this and the former Paradox are of importance, not only in themselves but to the rest of this Treatise, and are likely (in most Readers) to meet with indisposition enough to be receiv'd, I will subjoin in this place a couple of such Experiments, as will not, I hope, be unacceptable; that I devis'd, the one to confirm this second Paradox, and the other to prove the first.

Some of the Gentlemen now present may possibly remember, that about the end of the Year that preceded the two last, I brought into this place a certain new Instrument of Glass, whereby I made it appear, that the upper parts of water gravitate upon the lower; which I did by sinking a Body,

that was already under water, by pouring more water upon it.

But that Experiment belonging to other papers, I shall here substitute another perform'd by an Instrument, which though it makes not so fine a shew, may be more easily provided, and will as well as that other (though you were pleas'd to command that from me) serve to make out the same Truth; which I shall apply my self to do, as soon as I have, by an Improvement of the Expedient I am to propose, made good my late promise of confirming the second Paradox.

And before I can well draw an Argument from these Experiments, for either of the propositions to be prov'd by them, I must briefly repeat what I have elsewhere deliver'd already (on another occasion) touching the cause of the sinking

ing

ing of such Bubbles. Namely that the Bubble X, consisting of Glass, which is heavier, *Fig. 3.* in *specie* then Water; and Aire, which is lighter in *specie* then Water; and, if you please, also of Water itself, which is of the same *Specific Gravity* with Water; as long as this whole aggregate of several Bodys is lighter then an equal bulk of Water, it will float; but in case it grows heavier then so much water, it must, according to the known Laws of the *Hydrostaticks*, necessarily sink, (being not otherwise supported.) Now when there is any competent pressure (whether produc'd by weight or otherwise,) upon the water, in which this Bubble is for the most part immers'd, because the glass is a firm Body, & the water, though a Liquor, either suffers no compression, or but an inconsiderable one; the Aire included in the Bubble, being a springy
and

and very compressible Body, will
 be compell'd to shrink, and there-
 by possessing less Room, then it did
 before, the contiguous water will in-
 ceed in its place; which being a
 body above a thousand times heavier
 then air, the Bubble will there-
 by become heavier then an equall
 Bulk of water, and consequently
 will sink: but if that force or pres-
 sure be remov'd, the Imprison'd Aire
 will by its own Spring free it self
 from the intruding water: and the
 Aggregate of Bodys, that makes up the
 Bubble, being thereby grown lighter
 then an equal bulk of water, the subli-
 d bubble will presently emerge to the
 Top.

This Explication of the Causes of
 the sinking of Bubbles agrees, in some
 things, with the Doctrine of the Lear-
 ned Jesuites Kercher & Spottus, and some
 other writers, in the Account they give
 of

of those two Experiments that are commonly known by the name, the one of the Romanes, the other of the Florentine Experiments. But there are also particulars wherein I (who have never a recourse to a *funis Vacui*,) dissent from their Doctrine; the principles I go upon, having invited and assisted me to make that Experiment, afford me some new Phenomena, which agree not with their Opinions, but do with mine: but I forbear to mention them here, because they belong to other Papers; and for the same reason I omit some accession of Ludicrous Phenomena (as they call them,) which I remember I have sometimes added to those, which our Industrious Authors have already deduc'd from those Experiments.

These things being premis'd, I proceed to the confirmation of the second Paradox, by the following Experiment.

Take a long glass pipe, seal'd at one
 therwise exactly stop'd at one end and
 open at the other, whose Orifice if it
 be no wider, than that it may be conve-
 niently stop'd with a mans Thumb, the
 Tube will be the fitter to exhibit some
 other Phenomena, ch: Into this pipe
 pour such a quantity of common wa-
 ter, as that there may be a foot, or half
 a yard, or some other competent part
 left unfill'd, for the use to be by and by
 mention'd; Then having poiz'd a glass
 Bubble with a slender neck, in such a
 manner as that though it will keep at
 the Top of the water, yet a very little
 addition of weight will suffice to sink
 it, put this Bubble thus poiz'd into the
 Tube, where it will swim in the up-
 per part of the water, as long as it is
 let alone; but if you gently pour oyle of
 Turpentine upon it, (I say gently to
 avoid confounding the Liquors) you
 will perceive that for a while the
 Bubble

Bubble will continue where it was: but if you continue pouring in oyle, till it have attained a sufficient height above the water, (which it will be easie to perceive, because those two liquors will keep themselves distinct) & you shall see the Bubble subside till it fall to the Bottom, and continue there as long as the oyl remains at the height above the water.

The Reason of this Phenomenon, according to our Doctrine, is this, That the oyl of Turpentine, though a lighter Liquor then water, yet gravitates upon the subjaacent water, and by its pressure forces some of it into the cavity of the bubble at the open Office of its neck, whereby the Buble, which was before but very little less heavy then an equal Bulk of water, being by this accession made a little more heavy, must necessarily sink; and the cause of its submerision, namely the pressure of the
 Bubble oyle,

oyle, continuing, it must remain at the bottom.

And to confirm this explication, I shall add, that in case, by inclining the Tube or otherwise, you remove the Cylinder of oyl, or a competent part of it, (in case it were longer then was necessary,) the Bubble will again emerge to the Top of the water (for, as for the oyle, that is too light a Liquor to buoy it up;) which happens only because the pressure of the oyle upon the water being taken off, the Aire, by vertue of its own spring, is able to recover its former Expansion, and reduce the bubble to be as light as 'twas before.

And now we may proceed to that other Experiment, by which we lately promis'd to confirm the first Paradox. And in some regard this following Experiment has been prefer'd, as more strange, to that I have been reciting.

ting. For it seem'd much less impro-
bable, that of two Heterogeneous Li-
quors, the inferior should be press'd up-
on by the incumbent, which, though
lighter, kept in an intire body above it;
then that in water, which is a Homo-
geneous Liquor, and whose parts ming-
le most freely and exquisitely with one
another, the upper part should press
upon the lower, and that they will do
so, may appear by the Experiment it is
now time to subjoyn.

Provide a long Tube and a por'd
Bubble, as in the former Experiment,
then having pour'd water into the
Tube, till it reach above, or 6 Inches
(for a determinate height is no way
necessary) above the Bottom, cast
in the Bubble, which will not only
swim, but if you thrust it down into
the water it will of it self emerge to the
upper part of it. Wherefore take a
flender Wand, or a Wire, or a slender
glass

glass pipe, or any such Body that is long enough for your purpose, and with it having thrust the bubble beneath the Surface of the water, pour water slowly into the Tube (whose Cavity will not be near fill'd by the rod or wire) till it have attain'd a competent height, (which in my last Trials, was about a Foot, or half a Yard above the bubble;) and you shall see, that the bubble, which before endeavour'd to emerge, will by the additional weight of the incumbent water, be depress'd to the bottom of the Tube. After which you may safely remove the wire, or other body that kept it from rising. For as the weight of the Incumbent water was that which made it sinck, so that weight continuing on it, the bubble will continue at the bottom.

But yet it is not without cause, that we imploy a wire, or some such thing, in this Experiment, though we affirm

it to be only the weight of the Incam-
 bent water, that makes the Bubble sink.
 For if you should pour water into the
 Tube, to the height lately mention'd,
 or ev'n to a greater, if you did not make
 use of the Wire, it would not serve the
 turn, because that as fast as you pour
 in the water, the Bubble being left to
 it self, will rise together with it; and
 so, keeping always near the upper part
 of the water, it will never suffer the Li-
 quor to be so high above it, as it must
 be, before it can depress it. But to con-
 firm, that 'tis the weight of the Supe-
 rior water that smacks the Bubble, and
 keeps it at the Bottom; If you take out
 of the Tube a competent quantity of
 that Liquor, and so take off the pres-
 sure of it from the Bubble, this will
 presently, without any other help, be-
 gin to swim, and regain the upper part
 of the water; whence it may at plea-
 sure be precipitated, by pouring back
 into

into the Tube the water that was taken
out of it. And these Confirmations, ad-
ded to the former Proofs of the first and
second Paradoxes, being we conceive
sufficient to satisfy Impartial Readers
of the Truth of them, we should pre-
sently advance to the next Proposition,
if we did not think fit to interpose here
a *Scholium*, and so should not have
been surprised, that I have not done so.

SCHOLIUM

IT may perchance be wondered at,
why, since we lately mention'd our
having made some Tryals with oyle of
Tartar per Deliquium, we did not in the
present Experiment, instead of fair wa-
ter, make use of that; it being a very
much heavier Liquor, and (though it
may be incorporated with express'd
oyles running leable in such Tryals with
oyle of Turpentine. But to this I an-

swer,

swer, That ev'n in such slender pipes, as
 those made use of about the first Expe-
 riment, I found that Oyle of Tartar was
 ponderous enough to flow down, though
 slowly, into the Oyle of Turpentine at
 one side of the immensid Orifice, whilst
 the Oyle pass'd upwards by it along the
 other side of the pipe. And my know-
 ledge of this could not but make me
 a little wonder, That so Curious a per-
 son, as *Monsieur Paschall*, should some-
 where teach, That if a Tube of above
 14 foot long, and having its Orifice pla-
 ced 14 foot under water, be full of
 Quicksilver, the fluid Metal will not
 all run out at the Bottom of the pipe,
 though the Top of it be left open to
 the Aire, but will be stop'd at a foot
 high in the pipe. For the Impetus,
 that its fall will give it, must probably
 make it flow quite out of the pipe: And,
 not here to mention those Tryals of
 ours with Quicksilver and slender
 Tubes,

Tubes, that made me think this very improbable; if we consider that the Experiment will not succeed with much more favourable Circumstances, betwixt oyle of Turpentine and oyle of Tartar, though the heaviest of these two Liquors be many times lighter than quicksilver: It tempts me much to suspect, that *Monfrin Paschall* never actually made the Experiment, at least with a Tube as big as his Scheme would make one guess, but yet thought he might safely set it down, it being very consequent to those Principles, of whose Truth he was fully perswaded. And indeed, were it not for the impetus, the Quicksilver would acquire in falling from such a height, the Ratiocination were no way unworthy of him.

But Experiments that are but speculatively true, should be propos'd as such, and may oftentimes fail in practice, because there may intervene di-

vers other things capable of making them miscarry, which are overlook'd by the Speculator; that is wont to compute only the consequences of that particular thing which he principally considers; As in this case our Author seems not to have consider'd, that in such Tubes, as the Torricellian Experiment is wont to be made in, the largeness of them would make them unfit for this Tryal.

And I have known Ingenious men, that are very well exercis'd in making such Experiments, complain, that they could never make this of *Monfieur Paschall's* to succeed. In which attempts, that the size of the Tubes much contributed to the unsuccessfulness of the Tryals, I shall (without repeating what has been already intimated to that purpose) in the following part of this Discourse have

F

opportunity to manifest; and which
to adde as Illustrious a proof of this
our second Paradox, as almost any we
have yet given.

PARADOX III

That if a Body is contiguous to the surface of

water, it will be pressed down by the weight of the

water above it, and will sink, unless it be

supported by the surface of the water below it.

PROOF. Let a Body be contiguous to the surface of

water, and let the surface of the water be divided

into two parts, the one above the Body, and the

other below it. The weight of the water above the

Body will be directed downwards, and will press

the Body downwards. The weight of the water below

the Body will be directed upwards, and will support

the Body. If the weight of the water above the

Body be greater than the weight of the water below

it, the Body will sink. If the weight of the water

above it be equal to the weight of the water below

it, the Body will be suspended. If the weight of the

water above it be less than the weight of the water

below it, the Body will rise.

PARADOX III.

That if a Body contiguous to the water be altogether, or in part, lower than the highest level of the said water, the lower part of the Body will be press'd upward by the water that touches it beneath.

THIS may be prov'd by what has been already delivered in the Explication of the first Experiment: For where ever we conceive the lowest part of the Body, which is either totally, or in part, immers'd in water, to be there the imaginary Superficies being beneath the true Superficies; every part of that imaginary Superficies must be press'd upwards, by virtue of the weight of the water incumbent on

all the other parts of the same Superficies, and so that part of it, on which the immers'd Body chanceth to leane, must for the same Reason have an endeavour upwards. And if that Endeavour be stronger then that where-with the weight of the Body tends downwards, then (supposing there be no Accidental Impediment) the Body will be buoy'd or lifted up. And though the Body be heavier then so much water, and consequently will subside, yet that Endeavour upwards of the water, that touches its lower part, is onely rendred ineffectual to the raising or supporting the body, but not destroyed; the force of the heavy Body being from time to time resisted, and retarded by the water, as much as it would be if that Body were put into one Scale, and the weight of as much water, as is equal to it in bulk, were put into the other.

To

To confirm this, we may have recourse to what we said in the Explication of the second Experiment. *Fig. 1. 2.* For in case the slender pipe, wherein the water is kept suspended, be thrust deeper into the oyle, or in case there be more oyle pour'd into the Vessel, the water will be impell'd up higher into the pipe; which it would not be, if the oyle, though bulk for bulk a lighter Body, did not press against the lower Surface of the water, (where, alone, the two Liquors are contiguous, more forcibly then the water by its gravity tends downwards. And even when the Liquors rest in an *Equilibrium*, the oyle continually presses upwards, against the lower Surface of the water; since in that continual endeavour upwards, consists its constant resistance to the continual endeavour that the gravity of the water gives it to descend. And since the same Phenomenon hap-

pens; whether we suspend water in
oyle, as in the second Experiment, or
oyle in water, as in the first: It ap-
pears, that the proposition is as well
applicable to those cases, where the in-
flam'd Body is specifically heavier, as
to those where tis specifically lighter
than the subjacent fluid.

But a further and clearer proof of
this Doctrine will appear in the Ex-
plication of the next proposition. In
the mean time, to confirm that part of
our Discourse, where we mention'd the
Resistance made by the water to Bo-
dies that sink in it; Let us suppose,
In the annexed Figure, That
the pipe *E F* contains an oyle Fig. 4.
specifically heavier then water,
(as are the oyls of Clove, of Cin-
namon, or Cloves, and some others,)
and then, That the oyle in the pipe,
and the water without, being at rest
in an *Equilibrium*, the pipe be slowly
rais'd

rais'd towards the Top of the Vessel. 'Tis evident, from our former Doctrine, and from Experience too, that there will run out drops of oyle, which will fall from the bottom of the pipe, to that of the Vessel; but far more slowly then if they fell out of the same pipe in the Air.

Now to compute how much the pressure of the water against the lower parts of the drop amounts too, let us suppose the drop to be G , to whose lowermost part there is contiguous, in any assignable place where it falls, the imaginary Superficies $H I$. 'Tis evident, That if the drop of oyle were not there, its place would be supplied by an equal bulk of water; which being of the same specifick Gravity with the rest of the water in the Vessel, the Surface $H I$ would be laden every where alike; and consequently, no part of it would be displac'd. But now,

the drop of oyle being heavier then so much water, that part of the imaginary superficies, on which that drop leans, has more weight upon it, then any other equal part of the same Superficies; and consequently, will give place to the descending drop. And since the case of every other suppos'd Surface, at which the drop can be conceiv'd to arrive in its descent, will be the same with that of the Superficies HJ , it will for the Reason newly given, continue falling till it comes to the bottom of the Vessel which will suffer it to fall no further. And in case the drop G were not, as we suppose it, of a substance heavier in *specie* then watery but just equal to it, the contiguous part of the Superficies HJ would be neither more nor less charged then the other parts of the same Superficies; and the part lean'd on would be neither depress'd nor rais'd, but the drop G would

would continue in the same place. And so we may prove, (what is affirm'd by Archimedes, and other Hydrostatical Writers) That a Body equiponderant in specie to water, will rest in any assignable place of the water where 'tis put. And (to proceed further) since, if the drop G were of a matter but equiponderant to water it would not sink lower at all; no more then emerge; it follows, that though being heavier in specie then water, it will fall, yet the gravity upon whose account it falls, is no more then that by which it surmounts an equal bulk of water; (since, if it were not for that overplus, the resistance of the water would hinder it from falling at all :) and consequently, it looses in the water just as much of the weight it would have in the aire, as so much water weigh'd likewise in the same aire, would amount too.

Which is a Physicall Account of
 how that

that grand Theorem of the Hydrostatics, which I do not remember that I have seen made out in any Printed Book, both solidly and clearly, and The Learned Sir Isaac himself to whom the later Writers are wont to refer, having but an obscure (and not Physical) demonstration of it.

And, because this Theorem is not only very noble, but (as we elsewhere manifest) very useful, it will not be amiss to add, That it may easily be confirm'd by Experiment.

For if you take (for instance) a piece of Lead, and hang it by a Horse hair (that being suppos'd very near equal-ponderant to water) from one of the Scales of an exact Ballance, and, when you have put a just Counterpoize in the other Scale, suffer the Lead to sink in a vessel of water, till it be perfectly covered with it, but hangs freely in it, the counterpoize will very much

much preponderate, And, part of the Counterpoise being taken out till the Ballance be again reduc'd to an *Equilibrium*, you may easily (by subducting what you have taken out, and comparing it with the whole weight of the Lead in the aire) find what part of its weight it looses in the water. And then if you weigh any other piece of the same Lead, suppose a Lump of 12 ounces, and hang it by a Horse haire at one scale, you may be sure that by putting into the other scale a weight less by a twelfth part, (supposing Lead to water to be as twelve to one) that is eleven ounces, though the weights be far from an *Equilibrium* in the Aire, they will be reduc'd to it when the Lead is cover'd with water.

The pressure of water against the lower part of the Body immers'd in it may be confirm'd by adding; That we may thence deduce the cause of
 dum the

the emergency of wood and other Bodies lighter then water; which though a familiar Effect, I have not found its cause to have been so much as enquired into by many, nor perhaps to have been well rendered by any. If we suppose then that the pipe be almost fill'd, not with a sinking but a swimming oyle, as oyle of Turpentine, if, as in the first Experiment, the lower orifice be thrust under water, (to a far less depth then that of the oyle in the pipe) and the upper be slowly unstop'd, the oyl will (as we formerly declar'd) get out in drops at the bottom of the pipe. But to determine vvhy these drops, being quite cover'd and surrounded vvith vvater, and press'd by it as vvell downvvards as upvvards, should rather emerge then descend, I shall not content my self to say, that vvater in *specie* heavier then this kind of oyle; For, besides that in some cases (e're long to be mention'd) I have
made

made the water to depress ev'n this kind of oyle, and besides that 'tis not every piece of wood lighter in *specie* then water that will float upon water, how shallow soever it be: The Question is how this prepollent Gravity of the water comes to raise up the oyle, though there be perchance much more water, for it to break its way thorough, above it, then beneath it.

The Reason then of the emerfion of Lighter Bodies in heavier fluids, seems to be this, That the endeavour upwards of the water, contiguous to the lower part of the Body, is stronger then the endeavour downwards of the same Body, and the water incumbent on it. As, in the former Scheme, supposing the Drop *G* to be the oyle of Turpentine, and to touch the two imaginary and parallel plains *HJ*, *KL*, 'tis evident, that upon the lower part of the Drop, *N*, there is a greater pressure of water,

water, then upon the upper part of the same Drop, *M*: because that upon all the surface *N L*, there is but an uniform pressure of water *A K B L*, and upon all the parts of the surface *H I* there is a greater weight of water *A H B I*, except at the part *N*; for there the oyle *G*, being not so heavy as so much water, the oyle being expos'd to a greater pressure from beneath, then its own gravity (and that of the water incumbent on it) will enable it to resist, must necessarily give way and be impell'd upwards. And the case being the same between that and any other parallel plain, wheresoever we suppose it to begin its ascent, it must consequently be impell'd farther and farther upwards till it arrive at the Top; and there it will float upon the water. Or, (to Explicate the matter without Figures) when a specifically lighter Body is immers'd under water, it is press'd

against

against by two pillars of water; the one bearing against the upper, and the other against the lower part; and because the lengths of both these Pillars must be computed from the Top of the water, the lower part of the immers'd body must be press'd up on by a Pillar longer then the upper part by the thickness of the immers'd Body; and consequently must be press'd more upwards then downwards. And by how much the greater disparity of specifick Gravity there is betwixt the water and the emerging Body, by so much the swifter (*ceteris paribus*) it will ascend: because so much the more will there be of pressure upon all the other parts of the imaginary surface, then upon that part that happens to be contiguous to the Bottom of the ascending Body.

And upon the same Grounds we may give (what we have not yet met with)

a good solution of that Problem, propos'd by Hydrostatical Writers; why, if a Cylindrical stick be cut in two parts, the one as long again as the other, and both of them, having been detain'd under water at the same depth, be let go at the same time and permitted to emerge, the greater will rise faster then the lesser. For suppose one of these Bodies, as OP , to be two foot high, and the other, QR , to be half so much, and that the lowermost Surfaces of both be in the same Imaginary plane, parallel to the uppermost surface of the water, and three foot distant from it; in this case there will be against the lower part of each of the wooden Bodies a pressure, (from the laterally superior water) equal to that upon all the other parts of the Imaginary plain, whereto those Bodies are contiguous; But whereas upon the upper surface of the shorter Body, QR , there will lean

a pillar of water two foot high, the pillar of the same Liquor that will lean upon the Top of the taller Body, PO , will be but one foot high; as the attentive considerer will easily perceive. So that the wooden Bodys being lighter in *specie* then water, both of them will be impell'd upwards; but that compounded pillar, (if I may so call it) which consists of one foot of wood and two foot of water, will by its gravity more resist the being rais'd, then that which consists of two foot of wood and but one foot of water: so that the cause of the unequal celerity in the Ascension of these Bodys consists chiefly, (for I would neither overvalue nor exclude Concomitant Causes) that the difference of the pressure against the upper and lower part of each body respectively is greater in one then in the other.

And hence we may probably deduce a reason of what we often observe in the

Distillation of the oyles of Anniseeds, Cloves, and diverse Aromatick vegetables, in Lembacks by the intervention of water: for oftentimes, when the fire has not been well regulated, there will come over, besides the floating Oyle, a whitish water, which will not in a long time become cleare. And as we have elsewhere taught, That whiteness to proceed from the numerous reflections from the oily substance of the Concrete, by the heat of the fire broken into innumerable little Globuls, and dispers'd through the Body of the water; so the reason why this whiteness continues so long, seems to be chiefly (for I mention not such things as the great surfaces that these little Globuls have in respect of their Bulk) that, because of the exceeding minuteness of these Drops, the height of the water that presses upon the upper part, is almost equal to that of the water that presses

presses against the lower part ; so that the difference between these two pressures being inconsiderable, it has power to raise the Drops but very slowly, (in-
 somuch that upon this ground I devis'd a Menstruum, wherewith I could mingle oyle in drops so exceedingly minute, that, ev'n when there was but a few spoonfulls of the mixture, it would continue whitish for divers whole days together) though at length they will emerge ; and the sooner, because whilst they swim up and down, as they frequently chance to meet and run into one another, they compose greater Drops ; which are (for the Reason already given) less slowly impell'd up by the water : at the Top of which, the Chymist, after a due time, is wont to find new oyl floating. But whether this be any way applicable to the swimming of the insensible particles of corroded metals in *Aquafortis*, and other saline

Mentruums; I must not now stay to enquire.

One thing more there is, that I would point at before I dismiss this Paradox; Namely, that, for the same Reason we have all this while deduc'd, when the emergent drop, or any other Body, floats upon the Top of the water, it will sink just so far, (& no farther) till the immers'd part of the floating Body be equal in Bulke

Fig. 5. to as much water as is equal in weight to the whole Body. For suppose, in the annexed figure, X to be a Cube of wood three foot high and six pound in weight; this wood, being much heavier then Aire, will sink into the water, till it come to an imaginary superficies, XW ; where, having the position newly describ'd, it will necessarily acquiesce. For all the other equal parts of the Superficies, X, W, Q , being lean'd upon by pillars of water equal

qual in height to the part XA , or WB , if the whole weight of the wooden Cube be greater then that of as much water as is equal to the immers'd part, it must necessarily sink lower, because the subjacent part of the Surface (at K) will be more charg'd then any of the Rest. And, on the other side, if the Cube were lighter then as much water as that whose place the immers'd part takes up; it must by the greater pressure of the water upon the other parts of the imaginary superficies XW , then upon that contiguous to the wood, (as at K) be impell'd upward, till the pressure of the whole wood upon the part it leans on, be of the same degree with that of the rest of the water, upon the rest of the superficies: and consequently be the same with the water, whose place the immers'd part of it takes up. The lightness of that immers'd part, in respect of so much water, being re-

compenc'd by the weight of the im-
mers'd part, which is extant a-
bove the Superficies of the water.
And we see that when a piece of wood
falls into water, though, by the impe-
tus it acquires in falling, it passes
through divers imaginary plains that
lye beneath its due station; yet the
greater pressure, to which each of
those plains is expos'd in all its other
parts, then in that which is contiguous
to the Bottom of the wood, dos quick-
ly impel it up again, till, after some e-
mersions and subsidings, it rests at length
in such a position, as the newly expli-
cated Hydrostatical Theorem assigns
it.

SCHOLIUM.

S C H O L I U M.

THis Ingenious Proposition (about floating Bodys) is taught and prov'd after the manner of *Mathematicians*, by the most subtle *Archimedes* and his Commentators: and we have newly been endeavouring to manifest the Physical reason why it must be true. But partly because the Proposition ought to hold, not only in such intire and homogeneous Bodies as men exemplifie it in, (such as a piece of wood, or a Lump of wax) but in all Bodies, though of a concave figure, and made up of many Bodys of never so differing natures; (and perhaps some of them joyn'd together only by their superincumbency upon one another) and partly because that a Truth, which is one of the main and usefullest of the *Hydrostaticks*, and may be of so much impor-

importance to Navigation; has not yet (that I know of) been attempted to be demonstrated otherwise then upon Paper: it will not be amiss, for the satisfaction of such of those whom it may concern as are not vers'd in Mathematical Demonstrations, to add an Experiment which I made to prove it *Mechanically*; as exactly as is necessary for the satisfaction of such persons.

After (then) having imploy'd several Vessels, some of wood, some of Laton, and some of other materials, to compass what I desir'd; we found glasses to be the most commodious we could procure. And therefore filling a large and deep glass to a convenient height with fair water; we plac'd in it another deeper glass, shap'd like a Goblet or Tumbler, that it might be the fitter for swimming, and having furnish'd it first with Ballast, and then, for merriment sake,

sake, with a wooden Deck, by which a
 tall Mast, with a Sayle fasten'd to it,
 was kept upright; we freighted with
 wood, and by degrees pour'd Sand into
 it, till we had made it sink just to the
 Tops of certaine conspicuous marks,
 that we had fasten'd on the outside of
 the Glass in opposite parts thereof.
 Then observing how high the water
 reach'd in the larger Glass, (which by
 reason of the Vessels Transparency
 was easie to be seen) we carefully
 plac'd two or three marks in the
 same level with the Horizontal Sur-
 face of the water; and taking out
 the floating Vessel, as it was, with
 all that belong'd to it, and wiping the
 outside dry, we put it into a good paire
 of scales, and having found what it a-
 mounted to, we weigh'd in a compe-
 tently large Viol (first counterpoiz'd
 apart) so much water, (to a graine, or
 thereabouts,) and pouring this water
 into

into the large Glass above mentioned, we found it to reach to the marks that we had fasten'd to the outside of the Glass, and consequently to reach to the same height to which the weight of the floating glass, and all that was added to make it resemble a Ship, had made it arise to. By which Experiment (w^{ch} we tried, as to the essential parts of it, with Vessels of differing sizes, shapes; and loadings too, as Wood, Stone, Quick-silver, &c.) it appears, that the floating Vessel it self, with all that was in it, or supported by it, was equal in weight to, as much water as was equal in bulk to that part of the Vessel which was under water, suppos'd to be cut off from the extant part of the same vessel by a plain containing the Horizontal Surface of the water: since the weight of the floating Vessel, which rais'd up the water in the larger Vessel to the greatest height it attain'd, was the same with

with the weight of the water, which being pour'd into the larger vessel (when the other was taken out) rais'd the water therein to the same height. We may also obtaine the same end, by a somewhat differing way, (which is the best way in case the Vessels be too great, viz. to observe, first, by pouring in water out of a Bowle or Pail, or other Vessel of known capacity, as often as is necessary to fill the great Vessel, or Cistern, or Pond, to the Top, (or to any determinate height requir'd) and, next, letting out, or otherwise removing all that water, to put in its place the Vessel, whose weight is to be found out. Thirdly, to let, or poure in, water till the Vessel be afloat, and by its weight raise the External water to the height it had before: And lastly, to examine how much this water, that was last pour'd in, falls short in weight of

of the water that was in it at first; and afterwards remov'd. For this difference will give us the weight of as much water, as is equiponderate to the whole floating Vessel, whither small or great, with all that it either carries or sustains. The Hydrotatical Theorem we have been considering, and the Experiments whereby we have endeavour'd to confirm, or illustrate it, may (*Mutatis mutandis*) be apply'd to a Ship with all her Ballast, Lading, Guns, and Company; it holding generally true, That (to express the sence of the Proposition more briefly) *the weight of a floating Body, is equal to as much water, as its immers'd part takes up the room of.* Whence we might draw some Arguments in favour of the Learned Stevinus, (for whose sake it partly was that I annexed this *Scholium*) who, if I mis-remember not, does somewhere deduce as a Corollary from

from certain Hydrostatical ^{See PARADOX}
 Propositions, That a whole ^{the Fifth.}
 Ship, and all that belongs to it, and
 leans upon it, presses no more nor less
 upon the Bottom it swims over, then
 as much water, as is equal in bulk to
 that part of the Ship which is beneath
 the Surface of the water.

PARA.

~~THESE EXPERIMENTS ARE THE SAME AS IN THE PRECEDING~~

PARADOX IV.

*That in the Ascension of water in Pumps,
800, there needs nothing to raise the
Water, but a competent weight of an
External Fluid.*

THIS Proposition may be easily e-
nough deduc'd from the already
mention'd Experiments. But yet, for
further illustration and proof, vve vwill
add that which followvs.

Take a slender Glass-pipe, (such as
vvas us'd about the first Experiment)
and suck into it about the height of an
Inch of deeply tinted vwater; and, nim-
bly stopping the upper Orifice, im-
merse the lower part of the pipe into
a Glass half fill'd vwith such tinted
vwater, till the Surface of the Liquor
in

in the pipe be an inch (or as low as you would have it beneath that of the External water. Then pouring on oyle of Turpentine till it swim 3 or 4 Inches, or as high as you please above the vvater; loosen gently your finger from the upper Orifice of the pipe, to give the inclosed Aire a little intercourse vvith the External, and you shall see the tinged vvater in the pipe, to be impell'd up, not only higher then the Surface of the External vvater, but almost as high as that of the External oyl, through vvhich (it being transparent and colourless) the Red Liquor may be easily discern'd.

Now in this case it can't be pretended, That the ascent of the vvater in the pipe proceeds from Natures abhorrence of a *Vacuum*; since the pipe being full of aire, and its Orifice untopp'd, though the vvater should not ascend, no danger of a *Vacuum* vvould ensue; the aire and the vvater remaining contiguous as before.

The

The true Reason then of the ascent of the water, in our case, is but this, That upon all the other parts of the Imaginary Superficies, that passes by the immers'd Orifice of the pipe, there is a pressure partly of water, and partly of the oyle swimming upon that water, amounting to the pressure of 4 or 5 inches of water; whereas upon that part of the same superficies whereon the Liquor contain'd in the pipe leans, there is but the pressure of one inch of water, so that the parts near the immers'd Orifice must necessarily be thrust out of place by the other parts of water that are more press'd; till so much Liquor be impell'd up into the pipe as makes the pressure on that part of the Imaginary Superficies, as great as that of the oyle and water on any other equal part of it: and then, by Vertue of the *Equilibrium*, (often mention'd) the water will rise no further; and, by vertue of the same

same *Equilibrium*, it will rest a little beneath the Surface of the External oyle, because this last nam'd Liquor is lesse heavy, bulk for bulk, then water.

And by this we may be assisted to give a reason of the Ascension of water in ordinary sucking Pumps. For as the oyle of Turpentine, though a lighter Liquor then water, and not mingable with it, does, by leaning upon the Surface of the External water, press up the water within the pipe, to a far greater height then that of the External water it self: so the Aire, which, though a far lighter Liquor then oyle of Turpentine, reaches I know not how many Miles high, leaning upon the Surface of the water in a Well, would press it up into the Cylindrical Cavity of the Pump much higher then the External water it self reaches in the Well, if it were not hinder'd.

Now that which hinders it in the
 H Pump,

Pump, is either the Sucker, which
 fences the water in the Pump from the
 pressure of the External aire, or that
 pressure it self. And therefore, all
 that the drawing up of the Sucker needs
 to do, is, to free the water in the Pipe
 from the impediment to its Ascent,
 which was given it by the Suckers lean-
 ing on it, or the pillar of the Atmo-
 spheres being incumbent on it; as in our
 Experiment, the sides of the pipe do
 sufficiently protect the water in the
 pipe from any pressure of the Exter-
 nal oyle, that may oppose its ascent.

And lastly, as the water in our
 pipe was impell'd up so high, and no
 higher, that the Cylinder of water in
 the pipe was just able to ballance the
 pressure of the water and oyle without
 the pipe; so in Pumps, the water does
 rise but to a certain height, as about
 33 or 34 foot: and though you pump
 never so long, it will be rais'd no
 higher;

higher; because at that height the pressure of the water in the Pump, upon that part of the imaginary Superficies that passes by the lower Orifice of it, is the same with the pressure which other parts of that imaginary superficies sustaine from as much of the External water, and of the Atmosphere, as come to lean upon it.

That there may be cases wherein water may be rais'd by suction, not upon the Account of the weight of the air, but of its spring, I have elsewhere shovv'd; and having likewise in other places, endeavour'd to explicate more particularly the ascension of water in Pumps, what has been said already may suffice to be said in this place, where 'tis sufficient for me to have shovv'd. That whether or no the Ascension of water may have other causes, yet in the cases propos'd, it needs no more then the competent vweight of

an External Fluid, as is the Aire; whose not being devoid of gravity, the Cogency of our Experiments has brought even our Adversaries to grant us.

For confirmation of this, I will here add, because it now comes into my mind, (what might perhaps be elsewhere somewhat more properly mention'd) an Experiment that I did but lightly glance at in the Explication of the first, and the *Scholium* of the second Paradox.

In order to this I must advertise, That, whereas I there took notice, that some Ingenious men had complain'd, that, contrary to the Experiment propos'd by *Monsieur Paschall*, they were not at all able to keep *Mercury* suspended in Tubes, however very slender, though the lower end, were deeply immers'd in water, if both their ends were open: The Reasons of my doubting, whether our Ingenious Au-
thor

thor had ever made or seen the Experiment, were, not only that it had been unsuccessfully tryed, and seem'd to me unlikely to succeed in Tubes more slender then his appear'd; but because the Imperus, which falling quicksilver gains by the acceleration of motion it acquires in its descent, must in all probability be great enough to make it all run out at the bottom of a Tube, open at both ends, and fill'd with so ponderous a Liquor, though the Tube were very much shorter then that propos'd by *Monsieur Paschall*.

This advertisement I premise to intimate, that notwithstanding the hopelessness of the Experiment, as it had been propos'd and tried, I might have reason not to think it impossible to perform, by another way, the main thing desir'd; which was to keep Quicksilver suspended in a Tube, open at both ends, by the resistance of the subjacent

water. For by the Expedient, I am going to propose, I have been able to do it, even with a Liquor much lighter then water.

Finding then, that even a very short Cylinder of so ponderous a Solid, as Mercury, would, if it were once in falling, descend with an impetus not easy to be resisted by the subjacent Liquor, I thought upon the following Expedient to prevent this inconvenience. I took a slender pipe, the Diameter of whose Cavity was little above the sixth part of an Inch, and having suck'd in at the lower end of it somewhat lesse then half an inch of Quicksilver, and nimbly stopp'd the upper Orifice with my finger; I thrust the Quicksilver into, a deep glass of oyle of Turpentine, with a care not to unstop the upper Orifice, till the small Cylinder of quicksilver was 18 or 20 times its depth beneath the Surface of the oyle. For

by

by this means, when I unstopp'd the pipe, the Quicksilver needed not (as otherwise it would) begin to fall, as having a longer Cylinder then was requisite to make an *Æquilibration* with the other fluid. For by our Experiment the pressure of the oyle was already full as great, if not greater, against the lower part of the Mercurial Cylinder, as that which the weight of so short a Cylinder could exercise upon the contiguous and subjacent oyle. And accordingly, upon the removal of my finger, the Quicksilver did not run out, but remain suspended in the lower part of the pipe. And as, if I rais'd it towards the Superficies of the oyle, the Mercury would drop out for want of its wanted Counterpoise, so if I thrust the pipe deeper into the oyle, the increased pressure of the oyle would proportionably impell up the Mercury towards the higher parts of the pipe,

which being again a little, and but a
 little raised, the Quicksilver would fall
 down a little neerer the bottom of the
 pipe, and so, with a not displeasing
 spectacle, the ponderous Body of quick-
 silver was made sometimes to rise, and
 sometimes to fall; but still to float up-
 on the Surface of a Liquor, lighter than
 common Spirit of Wine is self. *proves*
 But, besides that, the Experiment,
 if the maker of it be not very careful,
 may easily enough miscarry, the diver-
 tement it gives, seldom proves last-
 ing; the oyle of Turpentine after a
 while insinuating in self between the
 sides of the pipe, and those of the short
 Cylinder of Mercury, and thereby
 disordering all. And therefore, though
 I here mention this Experiment, as I
 tryed it in oyle of Turpentine; because
 that is the Liquor I make use of all
 along these Paradoxes; and because also
 I would shew that a lighter fluid than
 water,

water, (and therefore why not aire, if its height be greatly enough increas'd) may by its weight and pressure, either keep the Mercury suspended in pipes, or even raise it in them. Yet I found water (wherever with I fill'd tall glasses) a fitter Liquor than oyle for the Experiment; in which though I sought, and found some other Phenomena, yet because they more properly belong to another place, I shall leave them unmention'd in this.

And since Experience shews us, that a Cylinder of Mercury, of about 30 Inches high, is equiponderant to a Cylinder of water of about 33 or 34 foot high; its very easie to conclude, That the weight of the External aire, which is able to raise and keep suspended 33 or 34 foot of water in a Pump, may do the like to 29 or 30 Inches of Quick-silver in the Torricellian Experiment.

PARA.

is, that the Force of the Hydrostatics, being



gives, that the Force of the Hydrostatics, being

PARADOX is, that the Force of the Hydrostatics, being

gives, that the Force of the Hydrostatics, being

That the Force of the Hydrostatics, being

is, that the Force of the Hydrostatics, being

suspended in the same height in several

Pipes, though these Pipes be of very

different Diameters.

THE contrary of this Proposition

is so confidently asserted and be-

lieved, by those Mathematicians, and

others, that favour the Doctrine of the

Schools. That this persuasion of

their seems to be the chief thing, that

has hindered men from acknowledging,

that the Quicksilver in the Torricellian

Experiment may be kept suspended by

the Counterpoize of the external air.

And a famous writer, that has lately

treated,

treated, as well of the Hydrostaticks, as of the Phenomena of the Torricellian Experiment, & finally, is much upon the falshood of our Paradox, That, laying aside all other Arguments, he contents himself to confute his Adversaries with our Demonstration (as he calls it) grounded on the quite contrary of what we here assert. For his Objection runs to this sense, That if it were the pressure of the External Aire, that kept the Quicksilver suspended in the newly mention'd experiment, the height would not be as Experience shews it is) be the same in all Cylindrical pipes, though of very differing Bore. For, supposing the height of the Mercurial Cylinder in a Tube of half an Inch Diameter, to be 29 Inches; it is plain, that a Mercurial Cylinder of the same height, and three Inches in Diameter, must weigh divers times as much as the former; and therefore the pressure of

of the External aire, being but one and the same, if it be a just Counterpoize to the greater Cylinder, it cannot be so to the less: and if it be able to keep the one suspended at 20 inches it must be able to keep the other suspended at a far greater height, which yet is contrary to experience. And indeed this Objection is so specious, That though I elsewhere have already answer'd it both by reason and Experience, as far forth as it concerns the Torricellian Experiment; Yet, to shew the mistake on which it is grounded, it may be very well worth while to make out our proposed Paradox, (as that whole truth will sufficiently disprove that error) by shewing both that the Assertion is true, and why it must be so.

Provide then a more than Fig. 6.
ordinarily wide mouth'd
Glass, cleer, and of a Convenient
depth; into which having put a con-
venient

venient quantity of water, deeply ting'd with Brazil or some other Pigment, fit to the Orifice a broad but thin Cork, in which, by burning or cutting, make divers round holes of very differing widenesses; into each of which you may thrust a glass Cylinder, open at both ends, and of a size fit for the hole that is to receive it; that so the several pipes may be imbrac'd by these several holes; And, as neare as you can, make them parallel to one another, and perpendicular to the superficies of the water into which they are to be immers'd. But we must not forget, that besides these holes, there is an aperture to be made in the same Corke (it matters not much of what figure or whereabouts) to receive the slender end of a glass Funnel; by which oyl may be convey'd into the vessel, when it is stopp'd with the Cork. And in the slender part of this Funnel

we

we use to put some Cotton-wool, to break the violence of the oyl that is to be pour'd in, which might else disorder the Experiment. All this being thus provided, and the Cork (furnish'd with its pipes) being fitted to the Orifice of the Vessel; if at the Funnel you pour in oyl of Turpentine, and place the Glass betwixt your eye and the Light; you may, through that transparent Liquor, perceive the Tincted water, to be impell'd up into all the pipes, and to rise uniformly in them. And when this tincted Liquor has attain'd to the height of two or three, or more Inches, above the lowermost Surface of the External oyl; if you remove the Funnel, (which yet you need not do, unless there be yet oyl in it,) you may plainly perceive the water to reach as high, in one of the smaller pipes, as in another three or four times as great; and yet the water

water in the several pipes, (as 'tis evident) is sustain'd, at that height above the level of the other water, by the pressure or counterpoise of the external oyle, which therefore being lighter in space than water, will have its Surface somewhat higher without the pipes, then that of the Fincked water within them. And if by the Aperture, that receives the Funnel, you immerse, almost to the Bottom of the oyle, the shorter leg of a slender glass Syphon, at whose longer Leg you procure by Suction the oyle to run out, you shall perceive, That, according as the depth and pressure of the External fluid decreases, so the water in the pipe will subside, and that uniformly, as well in the lesser as in the greater pipes.

The Reason of this is not difficult to be render'd by the Doctrine already deliver'd. For suppose *AB* to be the Surface of the water, both within
and

and without the pipes, before any oyle was poured on it: if we then suppose the oyle to be poured in through the Funnel, its lightness in respect of water, wherewith it will not mingle, will keep it from getting into the cavity of the pipes *L, M, N*; and therefore spreading it self on the outside of them above, it must necessarily, by its gravity, press down the Superficies of the external water, and impell up that liquor into the cavities of the pipes. And if we suppose the pouring on of the oyl to be continued till the uppermost surface of the oyl be raised to *G H*, and that of the external water depress'd to *I K*, (or thereabouts,) an imaginary plain passing along the lower Orifices of the pipes; I say, the tincted waters in the pipes ought to have their uppermost Surfaces in the same level, notwithstanding the great inequality of their Bores. For that part of the Surface

I K,

IK, which is comprehended within the Circular Office of the greatest pipe B, is no more charged by the incumbent water, then any other part, equal to that Circle of the same Imaginary Superficies, is by the water or oyle incumbent on it; and consequently, no more then the part comprehended within the circle of the small pipe N, is by the water contain'd in that small pipe; the external oyle having as much a greater height upon the Superficies IK, then the water within the pipe, as is requisite to make the two Liquors Counter-balance each other, notwithstanding the difference of their specific Gravities. And though the pipe D were twice as bigg, it would Charge the subjacent plain IK no more, then the pressure of the oyle on the other parts of the same Imaginary Surface is able to resist. And yet this pressure of the External oyle ought not

to be able to raise the water in the slender pipe N, higher then the Surface Q, in the same Level with the Surface O. For, if the water were higher in the small pipe; being a heavier Liquor then oyle, it must press upon that part of the Surface I K, it leans on, with greater force then the external oyle upon the other parts of the same plain I K; and therefore with greater force then the weight of the External oyle could resist. And consequently, the water in the slender pipe must subside, till its Surface be inferiour to that of the External oyle; since, till then, the difference of their specifick gravities cannot permit them to rest in an *Equilibrium*. To be short; It is all one, to the resistance of the external oyl, how wide the Cylinder is that it supports in the pipe; provided the height of it be not greater in respect of the height of the oyl, then the

the difference of the respective Gravities of those two Liquors requires. For, so long the pressure of the Cylinder of water will be no greater, on that part of the Imaginary Superficies which it leans upon, then the pressure of the external oyle will be on all the other parts of the same Superficies; and consequently, neither the one, nor the other of those Liquors will subside, but they will both rest in an *Equilibrium*.

But here it will not be amiss to note, First, that it is not necessary that the Glass Cylinders *L, M, N*, should be all of the same length, since, the lower Orifice being open, the water will rise to the same height within them, whether the parts immers'd under the water be exactly of the same length or no.

And Secondly, That throughout all this Discourse, and particularly in the

Explication of this Paradox, we suppose, either that the slenderest pipes, that are employ'd about these Experiments, are of a moderate size, and not exceeding small; Or that, in case they be very small, allowance be made in such pipes for this property, That water will rise in them to a greater Height, then can be attributed to the bare Counterpoize of either the water or the oyle, that impels it upwards and keeps it suspended. But this difference is of so little moment in our present Inquiries, That we may safely neglect it, (as hereafter we mean to do) now we have taken this notice of it for prevention of mistakes.

P A R A

And Secondly, That the Difference in the Heights, and the Difference in the

PARADOX VI

If a Body be plac'd under water, with its uppermost Surface parallel to the Horizon; how much water soever there may be on this or that side above the Body, the direct pressure sustain'd by the Body (if we now consider not the Lateral nor the receding pressure to which the Body may be expos'd if quite environ'd with water) is no more then that of a Column of water having the Horizontal superficies of the Body for its Basis, and the perpendicular depth of the water for its height.

And so likewise.

If the water that leans upon the Body be contain'd in pipes open at both ends; the pressure of the water is to be estima-

ted by the weight of a pillar of water, whose Base is equal to the lower Orifice of the pipe, (which we suppose to be parallel to the Horizon) and its height equal to a perpendicular reaching thence to the top of the water; though the pipe be much inclined towards the Horizon, or though it be irregularly shap'd, and much broader in some parts, then the said Orifice.

S ECOND, in the tenth Proposition of this Hydrostatical Elements, having propos'd in many general termes the former part of our Paradox, annexes to it a Demonstration to this purpose.

Having first supposed $MBCD$, to be a solid Rectangular figure of water, whose Base BF is parallel to the Horizon, and whose height GB is a perpendicular let fall from the uppermost Surface of the water to the lower-

lowermost; His Demonstration is this,



If the Bottom EF be charged with a greater weight then that of the water $GHFE$, that surplusage must come from the adjoyning water; therefore, if it be possible, let it be from the water $AGED$, & $HBCF$; which granted, the Bottom DE will likewise have a greater weight incumbent on it, upon the score of the neighbouring water $GHFE$, then that of the water $AGED$. And, the reason being the same in all the three cases, the Basis FC must susteine a greater weight, then that of the wa-

ter $HBCF$. And therefore the whole bottom DC , will have a greater weight incumbent on it, then that of the whole water $ABCD$; which yet ($ABCD$ being a rectangular Body) would be absurd. And by the same way of reasoning you may evince, That the Bottom EF sustains no less a weight, then that of the water $GHFE$. And so, since it sustains neither a greater weight, nor a less, it must sustain just as much weight as the Columne of water $GHFE$.

This Demonstration of the Learned *Stevinus* may well enough be admitted by a Naturalist (though, according to some Hypotheses touching the Cause and Nature of Gravity, it may faile of Mathematical exactness;) and by it may be confirm'd the first part of our propos'd Paradox. And some things annexed by *Stevinus* to this Demonstration, may be also apply'd to countenance

countenance the second. But because this is one of the noblest and usefullest Subjects of the Hydrostaticks, we think it worth while to illustrate, after our manner, each of the two parts of our Paradox by a sensible Experiment.

First then, Take a slender Glass pipe of an even Bore, turn'd up at one end like the annexed Syphon. Into this Syphon suck oyl of Turpentine Fig. 8. till the Liquor have fill'd the shorter leg, and be rais'd 2 or 3 Inches in the longer. Then nimbly stopping the upper Orifice with your finger, thrust the lower part of the Syphon so farre into a deep Glass full of water, That the Surface of the oyle in the longer leg of the pipe, may be but a little higher then that of the External water; and, upon the removal of your finger, you will find the Surface of the oyle to vary but little, or not at all, its former Station.

And

And as, if you then thrust the pipe a little deeper, you will see the oyle in the shorter leg to begin to be depress'd; so, if afterwards you gently raise the pipe toward the top of the water, you shall see the oyle not only regain its former station, but flow out by degrees in drops that will emerge to the Top of the water. Now, since the water was able, at first, to keep the oyl in the longer leg of the pipe, suspended no higher, then it would have been kept by a Cylinder of water equal to the Orifice of the shorter leg of the pipe, and reaching directly thence to the Top of the water; (as may be easily tried, by making a Syphon, where the shorter leg may be long enough to contain such a Cylinder of water to counterpoize the oyl in the longer;) & since, when once, by the raising of the pipe, the height of the incumbent water was less'n'd, the oyle did more then Counter-balance

Since it; (As appears by its flowing out of the Syphon;) we may well conclude; That, though there were in the Vessel a great deal of water, higher then the immers'd Orifice of the Syphon, (and it would be all one, though the Syphon were plac'd at the same depth in a pond or lake;) yet, of all that water, no more did gravitate upon the Orifice, then that which was plac'd directly over it; which was such a pillar of water, as the Paradox describes.

And, by the way, we may hence learn; That though water be not included in pipes, yet it may press as regularly upon a subjacent Body, as if it were. And therefore we may well enough conceive a pillar of water, in the free water itself, where there is nothing on any side, but the contiguous water, to bound the imaginary pillar.

But

But I had forgot to add, That the first part of our Paradox will hold, not only when the water, superior to the Body it presses upon, is free; but also, when it is included in Vessels of never so (seemingly) disadvantageous a shape. For, if you so frame the shorter leg of a Syphon, that it may expand its self into a funnel, like that of Fig. 6. employ'd about the proof of the foregoing (fifth) Paradox; (for which purpose the legs must be at a pretty distance from each other:) though you fill that Funnel with water, the oyle in the longer and slender leg of the Syphon will be able to resist the pressure of all the water, notwithstanding the breadth of the upper part of the funnel. So that, even in this case also, the Surface of the oyle in the longer leg, will be but a little higher then that of the water in the funnel.

For further Confirmation of this;

we caus'd to be made a Syphon, so shap'd, that one of the legs (which were parallel, and of the same Bore,) had in the midst of it a Sphere of Glass, save that it communicated with the upper and lower parts of the same leg.

In the uniform leg of the Syphon, we put a convenient quantity of oyle of Turpentine; and into the other, as much water as fill'd not only the lower part of it, but the Globular part too. And yet we did not find, that all this water was able to keep up the oyle in the uniform leg, at a greater height then if the leg that contain'd the water had been uniform too; as much of the water in the Globe, as was not directly over the lower Orifice of it, being supported by the lateral parts (if I may so call them) of the same Globe. And, if that leg were, instead of water, fill'd with oyle; and the uniform leg with water;

water; notwithstanding the far greater quantity of oyl, that was necessary to fill that leg, whereof the hollow Sphere was but a part; the water in the uniform leg would not be kept up, so much as to the same height with the oyle in the mishapen leg.

But to make this matter yet the more clear, we caus'd a Syphon to be made of the Figure express'd in the adjoining

Figur. 9.

Scheme; into which having poured a convenient quantity of Mercury, till it reach'd in the shorter leg *C D*, almost to the bottom of the Globulous part *B*, and in the longer leg *A B*, to an equal height: We afterwards, poured a sufficient quantity of water into the said longer leg *A B*, which drove away the Quicksilver, and impell'd it up in the shorter leg till it had half, or more then half, fill'd the Cavity of the Globular part *B*; which

yet

yet we did not wholly fill with Quick-
 silver, because the Tube *A B* was not
 long enough for that purpose;) and
 then we observ'd, that, notwithstanding
 the great weight of (that Body,
 which is of all Bodies, save one, the
 most Ponderous) Quicksilver, which
 was contain'd in the lower part of the
 same leg of the Syphon, the surface of
 the Quicksilver *H G*, was impell'd up
 as high by the water in the Leg *A B*,
 as the disparity of the specifick weights
 of those two Liquors (whereof one is
 about 14 times as heavy as the other)
 did require: So that it appear'd not,
 that, for all the great weight of
 Quicksilver, contain'd in the Globu-
 lous Cavity *E*, there press'd any more
 upon the slender and subjacent part
E C of that leg, then as much as was
 plac'd directly over the lower Orifice
 of the said Cavity *E*. So that the other,
 and lateral parts of that Mercury, be-
 ing

ing supported by the concave sides of the Glass, whereunto they were contiguous, the water in the leg *A B*, appear'd not any more press'd by the quicksilver, then if the leg *C D* had been, as well as the other, of an uniform bigness; and, by this means, if we had made the hollow Globe of a large Diameter, a small quantity of water, poured into the leg *A B*, might have been able to raise a quantity of quicksilver exceedingly much heavier then it self. But then so little water can raise the quicksilver, in so broad a pipe, but to an inconsiderable height.

To make out the second part of our Paradox by an Experiment, we took three Glass-pipes; the one *Fig. 10.* made like a Bolt-head, with a round Ball and two opposite Stems; the other was an irregular pipe, blown with an Elbow, where-with it made an Angle; and the third was

was as irregularly shap'd, as I could get it blown; being in some places much broader, and in some much narrower than the lower Orifice of it. And these two last nam'd pipes had their upper ends so inserted into holes, made fit for them in a broad piece of Cork; that, when they were immers'd, they made not right Angles, but very oblique ones, with the Horizontal Surface of the Liquor. The other Glass likewise, which consisted of a great Bubble, and two opposite pipes, was fastened to the same Cork, which having before hand been made fit for a wide mouth'd glass of a good depth, and half fill'd with water, was thrust as a stopple into the mouth of the said glass, so that the water ascended a pretty way into each of the three pipes by their lower Orifices, which as well as the upper we left open; Then a good quantity of oyle of Turpentine being

K

pour'd

pour'd into the same Vessel, through a funnel, the water was by the incumbent oyle impell'd up to the height of 2 or 3 Inches in each of the three pipes, Which argues, that, notwithstanding their being so unequal in bigness, and so irregular in shape, (inasmuch that we guess'd one of them was 10 or 12 times greater in one part, then in another, or then it was even at the Orifice) the water, contain'd in each of them, press'd upon its lower Orifice *no more* (I do not add, nor no less) then it would have done if it had been a Cylinder, having the Orifice for its Basis, and the perpendicular depth of the water and oyle above, for its height. For in case each of the pipes had contain'd but such a Cylinder of water, that water would nevertheless have had its uppermost Superficies at the same height: and on the other side, it would have been impell'd up beyond it, if its weight did
not

not as strongly endeavour to depress the immediately subjacent water, as the pressure of the External fluids endeavour'd to impel it up.

And since the height of the water was about the same in the several pipes, though two of them, being very much inclin'd, contain'd much more water than if they were erected: yet by the same way of reasoning we may gather, That the imaginary plain, passing by the immers'd Orifice of either of these inclining pipes, sustain'd no more of pressure, than it would have done from a shorter Cylinder of water if erected. And indeed, in all these cases, where a pipe either is broader in other places than at its lower Orifice, or inclin'd any way towards the Horizon, the weight of the contain'd Liquor is not all supported by the Liquor or the Body contiguous to the lower Orifice, but partly by the sides of the pipe it self. And

therefore if, when in a slender pipe you have brought a parcel of oyl of Turpentine to be in an *Equilibrium* with the External water, as in the Experiment belonging to the first Paradox ; If, I say, when this is done, you incline the pipe towards the sides of the Glass , You may indeed observe the Surface of the oyle in the pipe to be, as before, a little higher then that of the water without it: But you shall likewise see, That, though the Orifice of the pipe were not thrust deeper into the water, yet there will be a pretty deal of water got up into the pipe ; because the oyle not leaning now upon the water only, as it did before, but partly upon the water, and partly upon the pipe, its pressure upon the subjacent water is considerably lessen'd; and thereby the external water, whose pressure is not diminish'd too, is able to impel up the oyl, and intrude for a little way into the pipe. But if you re-erect the pipe, the pressure of the oyle being then again exerted

erted upon the subjacent water, it will be able to depress, and drive it again out of the Cavity of the pipe.

And to this agrees very well what we further try'd as follows: We caused 3 pipes to be blown (shap'd as the adjoyning Figures;) one having *Fig. 11.* in it divers acute Angles; the other being of a winding form like a *scrue* or *worm* of the Limbeck; and the third very irregularly crooked; and yet each of these pipes having all its crooked parts, and some of its streight & erected parts, fil'd with oyl of Turpentine; being thrust to a convenient depth under water and unstopp'd there, (after the manner already often declar'd) we found, that, according to our Paradox, the surface of the oyle in the pipe was higher than that of the water without it, as much as it would have been in case the pipe had been streight, (as we try'd by placing by the crookedest of them a streight pipe with oyle in it) though the quantity

of the oyle, in one of these pipes, were perhaps three times as much as would have suffic'd, if the pipe had been strait: So that this surplusage of oyle did not press upon the subjacent water, (for if it had done so, the oyle would have run out of the pipe.) And I remember, that lifting up as much of one of these crooked pipes, as I thought fit, somewhat above the Surface of the water; when the Superficies of the oyle in the pipe was not above half an inch higher then that of the water without it, I estimated that the crooked pillar of oyl, contain'd in that part of the *pipe* which was above the Surface of the water, was about 7 or 8 Inches long. So true it is, that the pressure of Liquors, contain'd in pipes, must be computed by the perpendicular that measures their height, what ever be their length or bigness.

Scholium.

S C H O L I U M.

THE Learned *Stevinus*, having demonstrated the Proposition we lately mention'd out of him, subjoyns divers conſectaries of which the truth hath been thought more questionable, then that of the Theorem it ſelf. And therefore he thought fit to add a kind of Appendix to make good a Paradox, which ſeems to amount to this. That If, in the Cover of a large Cylindrical Box, exactly cloſed, there be perpendicularly erected a Cylindrical Pipe open at both ends, and reaching to the Cavity of the Box; this Inſtrument being fill'd with water, the circular Baſis of it will ſuſteine a preſſure, equal to that of the breadth of the Baſis and height of the Pipe.

I chose thus to express this Theorem, [which might be, according to *Stevinus*, propos'd in more general terms,) because this way of expressing it will best suit with the subsequent Experiment, and may consequently facilitate the understanding of the Paradox. But though the Learned *Stevinus*'s aims were to be commended; who finding this Proposition doubted, seems to have had a great mind to give an Experimental Demonstration of it, and therefore proposes no less then five pragmatistical Examples (as he calls them) to make out the truth of what he asserts; yet in this he hath been somewhat unhappy, that that Experiment, which alone (for ought I can find) has been try'd of all the five, is rejected as incompetent, by those that profess to have purposely made tryal of it. And indeed, by reason of the difficulty of bringing them to a practical examen,

I have somewhat doubted whether or no this useful writer did ever make all those Tryals himself ; rather then set down the events, he suppos'd they must needs have ; as presuming his conjectures rightly deduc'd from a Demonstrative Truth. Wherefore though another of the Experiments, he proposes, be not free from difficulty, yet having, by the help of an Expedient, made it practicable, we are induc'd by its plainness and cleanness to prefer it to what else he proposes to the same purpose.

We provided then a vessel of Laton, of the figure express'd in the Scheme, and furnish- *See Fig. 12.* ed it with a loose Bottom *CD*, made of a flat piece of wood cover'd with a soft Bladder and greas'd on the lower side neer the edges, that leaning on the rim of wood *GH*, contiguous every where to the inside of the Laton, it

it might be easily lifted from off this Rim; and yet lye so close upon it, that the water should not be able to get out between them: And to the midst of this loose bottom was fastned a long string, of a good strength, for the use hereafter to be declared. The Instrument thus fitted, the water was poured in apace at the Top *A B*, which, by its weight pressing the false Bottom *C D* against the subjacent Rim *G H*, contributed to make the Vessel the more tight, and to hinder its own passing. The Vessel being fill'd with water we took the forementioned string, one of whose ends was fastned to *J*, the middle part of the loose Bottom; and, tying the other end *K* to the extremity of the Beam of a good pair of Scales, we put weights one after another into the opposite scale, till at length those weights lifted up the false Bottom *C D* from the Rim *G H*; and, consequently,
 lifted

lifted up the Incumbent water ; which
 presently after ran down between them.
 And having formerly, before we pon-
 red in any water, try'd what water
 would suffice to raise the Bottom CD ,
 when there was nothing but its own
 proper weight that was to be surmount-
 ed ; we found, by deducting that
 weight from the weight in the scale,
 and comparing the Residue with the
 weight of as much water, as the cavity
 of the broad, but very shallow
 Cylinder $BECHGDF$ would have
 alone (if there had been no water in the
 pipe AI) amounted to ; we found, I
 say, by comparing these particulars,
 that the pressure upon CD was by so
 very great odds more, then could have
 been attributed to the weight of so
 little water, as the Instrument pipe and
 all contain'd, in case the water had
 been in an uniform Cylinder, and con-
 sequently a very shallow one, of a Ba-
 sis

is as large as that of our Instrument;
 That we could not but look upon the
 success, as that, which though it did
 not answer what the reading of *Stevi-*
nius might make a man expect; yet
 may deserve to be further prosecuted,
 that whether or no the Paradox of *Ste-*
vinus (which not only some others,
 but the Learned Dr. *Wallis* himself
 question) will hold; the Inquiry he has
 started, may be so pursued, as to occa-
 sion some improvement of this part of
 Hydrostaticks: where, to define
 things with certainty, will perhaps be
 found a difficulter Task then at first
 glance one would think; both because
 divers speculative things must be taken
 into consideration, whose Theory has
 not perhaps yet been clear'd, and be-
 cause of the difficulty that will be
 found in practice by them that shall go
 about to make *Stevinus's* Experiments,
 or others of that sort with all requi-
 site

site Accurateness : As indeed, it is far easier to propose Experiments, which would in likelihood prove what we intend, in case they could be made, then to propose practicable Expedients how they may be made.

things at it ; as you will perceive
waters getting in between the
the Valve, and also **PARA:**

To which I think you will
you will find that the
Conformity to the
force of the
weight of
the water
the Valve
be lifted up above the
to overcome the resistance
ter, being now enabled
it is said near to high.



PARADOX VII.

That a Body immers'd in a Fluid, sustains a lateral pressure from the Fluid; and that increas'd, as the depth of the immers'd Body, beneath the Surface of the Fluid, increaseth.

THough I shall not wonder if this proposition seems strange enough to most Readers: yet I think I could make it out by several wayes, and particularly by one that is plain and easie, being but that which follows.

Take then a slender Glasse pipe
Fig. 13. (like that imployd about the first Experiment;) and cause it to be bent within two or three Inches of one end, so that the longer and the shorter

shorter legs, EF and FG , may make, as near as can be, a right Angle at F ; then dipping the Orifice of the shorter leg FG in oyle of Turpentine; suck into the Syphon (if I may so call it) as much of the Liquor, as will fill the shorter leg, and reach two or three Inches high in the longer; then, nimblely stopping the upper Orifice with your finger, immerse the lower part of the Glass under water, in such manner as that the longer leg EF may make, as to sense, right Angles with (AB) the Horizontal Surface, of the water, and the shorter leg FG may be so far depress'd under that Surface, That IK , the Superficies of the oyle in the longer leg, be but a little higher then AB , that of the external water. Then, removing your finger, you may observe, That the oyle in the Syphon will continue (with little or no change) in its former Itation. By which it appears that there
is

is a lateral pressure of the water against the oyle contiguous to G, the Orifice of the shorter leg of the pipe, since it is only that pressure that hinders the efflux of the oyl at that Orifice, notwithstanding the pressure of the perpendicular Cylinder of oyle that would drive it out.

And that this pressure of the perpendicular Cylinder doth really urge the oyle in the shorter leg to flow out; you may learn by slowly lifting the Syphon (without changing its former posture) towards the Surface of the water. For as the lower leg comes nearer and nearer to that Surface, (to which, as I newly intimated, it is still to be kept parallel) the oyle in the Horizontal leg will be driven out in drops, by the pressure of the other oyle in the perpendicular leg.

That likewise before you begin to raise the Syphon, the lateral pressure of

of the water against the lower Orifice of it is, at least in such Experiments, near about the same with what would be the perpendicular pressure of a Cylinder of water, reaching from the same Orifice G (or some part of it) to the top of the water, may be gather'd from hence, That the Surface of the oyle in the longer leg will be a little higher then that of the external water, as (by reason of the often mention'd comparative levity of the oyl) it would be, if we suppose, That a pipe of Glass of the same bore, and reaching to the top of the water, being fitted to the Orifice of the Horizontal Leg (as in the annex'd figure the Cylinder, G H) were fill'd with water.

And, to make out the latter part of our proposition, we need add no more, then that, if you plunge the Syphon deeper into the water, you shall find the oyle, by the Lateral pressure of the water, driven by degrees

L

quite

quite out of the shorter leg into the longer; and if you thrust it yet deeper, you may observe that the longer leg will admit a Cylinder of water, upon which that of oyle will swim; the whole oyle alone being unable to counterballance the lateral pressure of the water at so great a depth.

By which last circumstance, it appears, that water has also a lateral pressure against water it self; and that increas'd according to its depth; since otherwise the external water could not impel that in the Horizontal leg of the Syphon, into the perpendicular leg, though to doe so, it must surmount the weight or resistance of the whole cylinder of oyl, that must be hereby violently rais'd in the said perpendicular leg.

But if you gently raise the Syphon again, the lateral pressure of the water against the immers'd Orifice being diminish'd, (according as the distance

of

of that Orifice *G* from the Horizontal Surface, *AB*, comes to be lessen'd,) the prevalent oyle will drive out the water, first out of the Longer leg, and then out of the shorter, and will at length flow out in drops at the immers'd Orifice, and thence emerge to the Top of the water.

Besides, when the oyle in the Syphon does just counterballance the external water, if you keep the shorter leg parallel to the Surface of the water, and move the Orifice of it this way or that way, and place it nearer or further off from the middle or from the sides of the Glass, (provided you keep it always at the same depth under the water,) you'll find the oyl in the longer Leg to continue (as to sense) at the same height: Whence we may learn (what I have not yet found mention'd by any Writer,) That, ev'n in the mid'st of the water, we may suppose a

pillar of water, of a Basis equal to the side of an immers'd Body, (and reaching to the lowest part of it;) And that, though this Imaginary aqueous pillar, such as in our figure *G H*, be not included in any solid Body or stable superficies; nevertheless its lower parts will have a lateral pressure tending outwards, against the imaginary sides, from the weight of the water that is above these subjacent and lateral parts; and will have that pressure increas'd proportionably to the height to which the imaginary pillar reaches above them. Which observation, being duely noted and apply'd, may be of no mean use in the explication of divers Hydrostatical phenomena.

And lastly if, in stead of holding *E F*, the longer leg of our Syphon, perpendicular, (and, consequently, the shorter parallel to the Horizon,) you variously incline the former, so as to bring

bring it to make an obtuse or an Acute Angle with the superficies of the water AB ; though by this means the shorter and immers'd leg, FG , will in Situation sometimes respect the Bottom, and sometimes the top of the Glass: yet in all these oblique situations of this leg, and the immers'd Orifice of it G , the oblique presture of the water will so much depend upon the height of the Surface of the Liquor above the Orifice, and so much conform to the observations already deliver'd, That you shall still see the surface of the oyle IK , in the longer pipe, to be a little, and but a little superiour to that of the external water, AB , and so the *Equilibrium* betwixt the Liquor, or Liquors, within the Syphon, and the water without it, will ev'n in this case also be maintain'd.

S C H O L I U M.

REmembring on this Occasion an Experiment, which though it do not shew what the precise quantity of Lateral pressure is, that the lower parts of the fluid may sustain from the more elevated; yet it may confirm the foregoing Paradox, and by its Phænomena afford some hints that may render it not unacceptable; I shall subjoyn it, as I set it down not long after I devis'd it.

In the first place then, there was made a glass Bubble with a slender neck; and (in a word) of the figure express'd in the annex'd *Fig. 14.* Scheme; This Bubble I caus'd to be so poys'd, That, though it would float upon the water, yet the addition of a weight small enough would suffice to make it sinck. This

This done, I provided a very large wide mouth'd Glass, and caus'd to be fitted to it, as exactly as I could, a stopple of Cork, which being strongly thrust in, would not easily be lifted up. In the middle of this Cork there was burn'd, with a heated instrument, a round hole; through which was thrust a long slender pipe of Glass; so that the lower end of it was a pretty way beneath the Cork, and the upper part of it was, as near as could be, at right Angles with the upper part of the said Cork. And in an other part of the stopple, near the edge, there was made another round hole, into which was likewise thrust another small pipe; whose lower part reach'd also a pretty way beneath the Cork; but its upper part was but about two or three Inches high; and the Orifice of this upper part was carefully clos'd with a stopple and Cement. Then the glass vessel be-

ing fill'd with water, and the pois'd Bubble being made to float upon it, the stopple or cover of the great glass vessel was put on, and made fast with a close Cement, that nothing might get in or out of the vessel, but at the long slender pipe; which was fastned into the Cork (as was also the shorter pipe) not only by its own fitness to the hole, it pass'd through, but by a sufficient Quantity of the same Cement, carefully apply'd to stop all crevisses.

The Instrument thus prepar'd, (and inclin'd this or that way, till the floating Bubble was at a good distance from that end of the long pipe, which reached a pretty way downwards beneath the Surface of the water,) we began to pour in some of that Liquor at the open Orifice of the pipe *B F*; and, the mouth of the Vessel being exactly stopp'd, the water for want of another

place

place to receive it, ascended into the pipe through which it had fallen before. And, if I held my hand when the water I had pour'd in was able to reach but to a small height in the Cylinder, as for instance, to the Superficies *J*; the Bubble *X* would yet continue floating. But if I continued pouring till the water in the pipe had attain'd to a considerable height above the Surface of that in the Vessel, as if it reach'd to *K*; then the Bubble *X* would presently sink to the bottom of the Vessel; and there continue, as long as as the water continued at so great a height in the pipe *BF*.

This Experiment will not only teach us, That the upper parts of the water gravitate upon those that are under them, but (which is the thing we are now to confirm) That in a Vessel, that is full, all the lower parts are press'd by the upper, though these lower be

not

not directly beneath the upper, but at
 side of them, and perhaps at a good di-
 stance from the Line in which they di-
 rectly press: These things, I say, may
 be made out by our Experiment. For
 the Addition of the Cylinder of water
 KJ , in the pipe EF , makes the Bub-
 ble X subside; as the force or pressure
 of any other heavy body upon the wa-
 ter in the vessel would do. And since
 (as may be gather'd from the Reason
 formerly given (in the Proof of the se-
 cond Paradox) of the sinking of pois'd
 Bubbles) the included aire in our Bub-
 ble was notably compress'd; it will
 follow, that the Cylinder of water, KI ,
 did press the subjacent water in the
 Vessel. For, without so doing, it could
 not be able to compress the aire in the
 Bubble. And since the said Bubble
 did not swimme directly under or near
 the pipe EF ; but at one side of it, and
 at a pretty distance from it, nay and
 floated

floated above the lower Orifice, *F*, of the pipe; 'tis evident that that Aqueous Cylinder, *JK*, does not only press upon the water, or other Bodies that are directly under it; but upon those also that are laterally situated in respect of it, provided they be inferior to it.

And, according to this Doctrine, we may conceive, that every assignable part of the sides of the Vessel does sustaine a pressure, encreas'd by the encrease of that parts depth under water, and according to the largeness of the said part. And therefore, if any part were so weak, as that it would be easily beaten out or broken by a weight equal to the Cylinder *IK*, (making always a due abatement for the obliquity of the pressure) it would not be fit to be a part of our Vessel: Nay the Cork it self, though it be above the Surface of the water in the Vessel; yet because the water in the pipe is higher then it, each of its

parts

parts resists a considerable pressure proportionate to its particular bigness, and to the height of the water in the pipe. And therefore, if the Cork be not well stopp'd in, it may be lifted up by the pressure of the water in the pipe, if that be fill'd to a good height. And if the Cement be not good and close, the water will (not without noise) make it self a passage through it. And if the stopple *G*, of the shorter pipe *G H*, (which is plac'd there likewise to illustrate the present conjecture) do not firmly close the Orifice of it, it may be forced out, not without violence and noise. And, for further satisfaction, if, in stead of the stopple *G*, you close the Orifice with your finger, you shall find it press'd upwards as strongly, as it would be press'd downwards by the weight of a Cylinder of water of the breadth of the pipe, and of a not inconsiderable height, (for 'tis not easie to determine precisely, what

what height :) so that (to be short) in the fluid Body, we made our tryal with, the pressure of the Superior parts was communicated, not onely to those that were plac'd *directly* under them, but ev'n to those that were but *obliquely* so, and at a distance from them.

I had forgot to confirm, that it was the pressure of the superiour parts of the water, that made our floating bubble sinck, by such another circumstance as I took notice of in some of the former Experiments; viz. that, when it lay quietly at the bottom of the Vessel, if by inclining the Instrument we pour'd off as much of the water in the pipe, *EF*, as suffic'd competently to diminish its height above the water in the Vessel *ABCD*, the air in the bubble, finding its former pressure alleviated, would presently expand it self, and make the bubble emerge. And to show, That the very oblique pressure wth the bubble

ble sustain'd from the water in the pipe, was not overmuch differing from that which it would have sustain'd from an External force, or from the weight of water plac'd directly over it; I caus'd two such bubbles to be pois'd, and having put each of them into a long Cylindrical Glass, open above, and fill'd with water, upon which it floated, if we thrust it down a little way it would (agreeably to what hath been *See the Proof of the 11. Paradox.* above related) ascend again; so that we were forc'd to thrust it down to a good depth, before the pressure of the incumbent water was great enough to make it subside.

And perhaps it will not be impertinent to take notice, before we conclude, how the pressure of such differing fluids, as aire and water, may be communicated to one another. For having sometimes forborn to fill the Vessel *A B C D* quite full of water, so that,

that, when the Cork was fitted to it, there remain'd in it a pretty quantity of aire, (as between the Surface LM , and the Cork) nevertheless, if the stopple or cork were very closely put in, the pressure of the water that was afterwards poured into the pipe EF , from F to K , would make the bubble sink, little otherwise, for ought I took notice of, then if the Vessel had been perfectly fill'd with water; the aire (above LM ,) that was both imprisoned and compress'd, communicating the pressure it receiv'd to the water contiguous to it;

PARA

PARADOX VIII.

*That water may be made as well to depress
a Body lighter then it self, as to buoy
it up.*

HOW strange soever this may seem,
to those that are prepossess'd with
the vulgar Notions about gravity and
levity: It need not be marvel'd at, by
them that have consider'd what has
been already deliver'd. For since, in
Fluid Bodies, the upper parts press
upon the lower, and upon other bodies
that lie beneath them. And since,
when a Body is unequally press'd
by others, whether lighter or heavier
then it self, it must necessarily be thrust
out of that place, where it is more
press'd

press'd, to that where 'tis less press'd ;
 If that a parcel of oyle be by a contri-
 vance so exposed to the water, as that
 the water presses against its upper Su-
 perficies, and not against the undermost
 or lateral parts of it ; If we suppose
 that there is nothing (whose pressure is
 not inferiour to that of the water) to
 hinder its descent, (supposing, withal,
 that the oyle and water cannot pass by
 one another ; for which cause, we make
 use of a slender pipe ;) the oyle must ne-
 cessarily give way downwards, and con-
 sequently be depress'd and not buoy'd
 up. This is easily exemplified by the
 following Experiment.

Take a slender Glass Syphon *E F*
G H, of the bore we have often
 mention'd, whose shorter leg *G H*
 may be about 3 or 4 Inches long,
 and as parallel as the Artificer can make
 it to the longer *E F*; dip the shorter leg
 in oyle of Turpentine, till the oyle

M

quite

quite full, the shorter leg, and reach to an equal height in the longer, as from *F* to *J*. Then stopping the Orifice *E* of the longer leg with your finger, and immersing the replenish'd part of the Syphon about an inch under water, you shall perceive that as you thrust it lower and lower, upon the removal of your finger, the oyle in the shorter leg will be made to sink about an inch or somewhat more: and as afterwards you thrust the pipe deeper, the oyle in the shorter leg will, by the weight of the incumbent water, *H. K.* be driven downward more and more, till it come to the very bottom of the shorter leg: whence, by continuing the immersion, you may impel it into the longer. The cause of which Phenomenon I suppose to be already clearly enough assign'd, to make it needless to add any thing here about it.

It remains, that, before I proceed

to

to the next proposition, I add; That;
 to Exemplifie at once three Paradoxes,
 (both this, and the next fore-
 going, and the second) I caus'd Fig. 16.
 to be made a slender Glass-pipe, of
 the Figure express'd in the annexed
 Scheme, and having, by the lower
 Orifice L , suck'd into it as much oyle
 of Turpentine, as reach'd in the lon-
 gest leg, NO , as high as the Top of
 the other part of the Glass; (namely,
 to the part P , in the same level with
 the Orifice L ;) I first stopp'd the up-
 per Orifice of it, O , with my finger.
 And then, thrusting it as before un-
 der water to a convenient depth, upon
 the removal of my finger, the Exter-
 nal water did first drive away the oyle
 that was in LM , that part of the
 crooked pipe which was parallel to the
 Horizon; then it depress'd the same
 oyle to the bottom of the shorter leg,
 that is from M to N : And lastly,

it impell'd it all up into the longer leg NPO , to what height I thought fit. So that the oyle was press'd by the water both laterally, downwards, and upwards: the causes of which are easily deducible from the Doctrine already deliver'd.

PARA.



PARADOX IX.

*That, what ever is said of positive Levity,
a parcel of oyle lighter then water, may
be kept in water without ascending
in it.*

TO make out what I have to re-
present about this Paradox the
more intelligible, the best way per-
haps will be to set down the Consi-
derations that induc'd me to judge the
thing it pretends to feasible. And in
order to this, it would be expedient to
consider, why it is that a Body lighter
in *specie* then water, being plac'd re-
solv'd to touch beneath the Superficies of
that Liquor, will rather emerge to the
M 3 Top;

Top, then sink to the bottom of it; if we had not already consider'd that problem in the Explication of the third Paradox. But being now allow'd to apply to our present purpose what hath been there deliver'd, I shall forthwith subjoyne, That 'twas easie enough for me to collect from hence, that the Reason why it seems not possible, That a parcel of oyle lighter then water, should without violence be kept from emerging to the Top of it, being this, *That since the Surface of a Vessel full of standing water is (Physically speaking) Horizontal, the water that presses against the lower part of the immers'd Body, must needs be deeper then that which presses against the upper.* If I could so order the matter that the water that leans upon the upper part of the Body should by being higher then the level of the rest of the water have a height great enough to ballance that which presses

E M

against

against the lower, (and the Bodies not
 find places by passing one by the other)
 the oyle might be kept suspended be-
 twixt two parcels of water. and so
 we tried to reduce this to practice, but took
 the following course; having suck'd
 into a slender pipe (such as that im-
 ploy'd about the first experiment) about
 an Inch of water, and kept it suspended
 there by stopping the Orifice of the
 pipe; I thrust the lower part of the
 pipe about two inches beneath the Sur-
 face of some oyl of Turpentine which
 to make the effect the clearer, I some-
 times finger deeply with Copper and then
 removing my finger, the oyle being
 press'd against the narrower Orifice
 with a greater force, then the weight
 of so little suspended water could re-
 sist, that oyle was impell'd into the
 lower part of the pipe to the height of
 near an inch; and then again I stopp'd
 the upper Orifice of the pipe with my
 finger,

finger, and thereby keeping both the Liquors suspended in it, I thrust the pipe into a Glass full of water, three or four inches beneath the Surface of it; and then (for the Reason just now given) the water, upon the Removal of my finger, will press in at the lower Orifice of the pipe, and impell up the *Fig. 17.* oyle, till they come to such a station, as that express'd in the annex'd Scheme: where *P Q* is the water, newly impell'd up into the pipe, *Q R* is the oyl, and *R S* the water that was at first suck'd into the pipe. For in this station, these three liquors do altogether as much gravitate upon the part *P*, as the incumbent water alone does upon the other parts of the imaginary superficies *G H*; and yet the oyle, *Q R*, does not ascend, because the diffuence of the water, *R S*, being hindred by the sides of the pipe, its superficies, *T S*, is higher then *A D*, the Super-

Superficies of the rest of the water; by which means the incumbent water may be brought to have upon the upper part *R* of the oleous Cylinder, as great a pressure as that of the water, that endeavours to impel upwards the lower part *Q* of the same suspended Cylinder of oyle.

[Faint, illegible text]

[Faint, illegible text]

PARA

[Faint, illegible text]

[Faint, illegible text]

[Faint, illegible text]

Subjunctive of the verb of the water;



as, namely, I suppose it to be a part of

the water.

PARADOX.

That the cause of the Ascension of water in Syphons, and of its flowing through them, may be explicated without having a recourse to nature's abhorrency of a Vacuum.

Both Philosophers and Mathematicians, having too generally confest themselves reduc'd to fly to a *fuga vacui*, for an account of the cause of the running of water and other Liquors through Syphons. And ev'n those moderns, that admit a *Vacuum*, having (as far as I have met with) either left the Phenomenon unexplicated, or endeavour'd to explain it by disputable Notions: I think the Curious much oblig'd to

Monsieur

Monsieur *Paschal* for having ingeniously endeavour'd to shew, That this difficult Probleme need not reduce us to have recourse to a *fuga vacui*. And indeed his Explication of the motion of water in Syphons, seems to me so consonant to Hydrostatical principles, that I think it not necessary to alter any thing in it. But as for the experiment he propounds to justify his Ratiocination, I fear his Readers will scarce be much invited to attempt it. For, besides that it requires a great quantity of Quicksilver; and a new kind of Syphon, 15 or 20 foot long; the Vessels of Quicksilver must be plac'd 6 or 7 yards under water, that is, at so great a depth, that I doubt whether men, that are not divers, will be able conveniently to observe the progress of the Tryal.

Wherefore we will substitute a way, which may be try'd in a glass Tube, not

two foot deep, by the help of another peculiarly contriv'd glass, to be prepar'd by a skilful hand. Provide then a glass Tube $A B C D$, of a good wideness, and half a yard or more in depth; provide also a Syphon of two legs $F K$, and $K G$, whereunto is joyn'd (at the upper part of the Syphon) a pipe $E K$, in such manner, as that the Cavity of the pipe communicates with the cavities of the syphon; so that if you should pour in water at E , it would run out at F and G . To each of the two Legs of this new Syphon, must be ty'd with a string a pipe of Glass, I and H , seal'd at one end, and open at the other; at which it admits a good part of the leg of the Syphon to which it is fastned, and which leg must reach a pretty way beneath the Surface of the water, wherewith the said pipe is to be almost fill'd. But as one of these legs is longer then

Fig. 18.

then the other, so the surface of the water in the suspended pipe *I*, that is fastned to the shorter leg *K F*, must be higher (that is, nearer to *K* or *A B*) then the surface of the water in the pipe *H*, suspended from the longer leg *K G*; that (according to what is usual in Syphons) the water may run from a higher vessel to a lower.

All things being thus provided; and the pipe *E K* being held, or otherwise made fast that it may not be mov'd; you must gently poure oyle of Turpentine into the Tube *A B C D*, (which, if you have not much oyle, you may before hand fill with water till the liquor reach near the Bottom of the suspended pipes, as to the superficies *X Z*) till it reach higher then the top of the Syphon *F K G*, (whose Orifice *E* you may, if you please, in the mean time close with your finger or otherwise, and afterwards unstop)
and

and then the coyle preſſing upon the water will make it aſcend into the legs of the Syphon; and paſs through it, out of the uppermoſt veſſel *F*; into the lowermoſt *H*; and if the veſſel *F* were ſupply'd with water, the coarſe of the water through the Syphon would continue longer, then here (by reaſon of the paucity of water) it can do.

Now in this Experiment we manifeſtly ſee the water made to make its courſe through the legs of a Syphon from a higher veſſel into a lower; and yet the top of the Syphon being perforated at *K*, the aire has free acceſs to each of the legs of it; through the hollow pipe *E K* which communicates with them both. So that, in our caſe, whether there is no danger of a *Vacuum*, though the water ſhould run through the Syphon the fear of a *Vacuum* cannot with any ſhew of Reaſon be pretended to be the cauſe of its running. Wherefore

fore we must seek out some other.

And it will not be very difficult to find, that 'tis partly the pressure of the oyle, and partly the contrivance and situation of the vessels; if we will but consider the matter somewhat more atently. For the oyle, that reaches much higher then K, and consequently then the leggs of the Syphon, presses upon the surface of the External water, in each of the suspended pipes I and H. I say the *External water*, because the oyle floating upon the water, and the Orifices of both the leggs F and G being immers'd under the water, the oyle has no access to the cavity of either of those leggs. Wherefore, since the oyle gravitates upon the water without the leggs, and not upon that within them, and since its height above the water is great enough to press up the water into the Cavity of the leggs of the Syphon, and impel it as high as K, the
water

water must by that pressure be made to ascend.

And this raising of the water happening at first in both legs, (for the cause is in both the same) there will be a kind of conflict about K betwixt the two ascending portions of water; and therefore we will now examine which must prevaile.

And if we consider, That the pressure, sustained by the two parcels of water in the suspended pipes I and H, depends upon the height of the oyle that presses upon them respectively; it may seem (at the first view) That the water should be driven out of the lower vessel into the higher. For if we suppose that part of the shorter leg that is unimmers'd under water to be 6 Inches long, & the unimmers'd part of the longer leg to be seaven Inches; because the surface of the water in the vessel I, is an Inch higher, then that of the

the

the water in the vessel *H*, it will follow,
That there is a greater pressure upon
the water, whereinto the longer leg is
dip'd, by the weight of an Inch of oyle:
so that that liquor being an inch higher
upon the surface of the water in the
pipe *H*, then upon that in the pipe *I*,
it seems that the water ought rather to
be impell'd from *H* towards *K*, then
from *I* towards *K*.

But then we must consider, That,
though the descent of the water in the
leg *G*, be more resisted then that in
the other leg, by as much pressure as
the weight of an Inch of oyle can amount
to; Yet being longer by an Inch then
the water in the leg *F*, it tends down-
wards more strongly by the weight of an
Inch of water, by which length it exceeds
the water in the opposite leg. So that
an inch of water being (*ceteris paribus*)
heavier then an Inch of oyle; the wa-
ter in the longer leg, notwithstanding

the greater resistance of the external
 oyle, has a stronger endeavour down-
 wards, then has the water in the shorter
 leg; though the descent of this be resist-
 ed but by a depth of oyl less by an Inch.
 So that all things computed, the mo-
 tion must be made towards that way
 where the endeavour is most forcible;
 and consequently the course of the wa-
 ter must be from the upper vessel, and
 the shorter leg, into the longer leg, and
 so into the lower vessel.

The application of this to what
 happens in Syphons is obvious enough.
 For, when once the water is brought
 to run through a Syphon, the aire
 (which is a fluid and has some gravity,
 and has no access into the cavity of the
 Syphon,) must necessarily gravitate
 upon the water whereinto the legs of
 the Syphon are dip'd, and not upon
 that which is within the Syphon: and
 consequently, though the incumbent

aire have somewhat a greater height
 upon the water in the lower vessel, then
 upon that in the upper; yet the gra-
 vitation it thereby exercises upon the
 former more then upon the latter, be-
 ing very inconsiderable, the water in
 the longer leg much preponderating
 (by reason of its length) the water in
 the shorter leg, the efflux must be out
 of that leg, and not out of the other.
 And the pressure of the External aire
 being able to raise water (as we find by
 sucking Pumps) to a far greater height,
 then that of the shorter leg of the Sy-
 phon; the efflux will continue, for the
 same reason, till the exhaustion of the
 water, or some other circumstance, al-
 ter the case. But, if the legs of the Sy-
 phon should exceed 34 or 35 foot of
 perpendicular altitude; the water
 would not flow through it; the
 pressure of the external aire being unable, (as

has been elsewhere declared,) to raise water to such a height. And if a hole being made at the top of a Syphon, that hole should be unstopp'd while the water is running, the course of it would presently cease. For, in that case, the air would gravitate upon the water, as well within as without the cavity of the Syphon; and so the water in each leg would, by its own weight, fall back into the vessel belonging to it.

But because this last circumstance, though clearly deducible from Hydrostatical principles and Experiments, has not, that I know of, been verified by particular Tryals, I caus'd two Syphons to be made, the one of Tin, the other of Glass; each of which had, at the upper part of the flexure, a small round hole or socket, which I could stop and unstop, at pleasure, with the pulp of my finger. So that, when

when the water was running through the Syphon, in case I remov'd my finger, the water would presently fall, partly into one of the subjacent vessels, and partly into the other. And if the legs of the Syphon were so unequal in length, that the water in the one had a far greater height (or depth) then in the other; there seem'd to be, when the liquor began to take its course through the Syphon, some light pressure from the external aire upon the finger, wherewith I stopp'd the Orifice of the socket made at the flexure.

And on this occasion I will add, what I more then once try'd; to shew, at how very minute a passage the pressure of the External aire may be communicated, to Bodyes fitted to receive it. For, having for this purpose stopp'd the orifice of one of the above mentioned Syphons, (instead of doing it with my finger,) with a piece of oyl'd paper,

carefully fastned with Cement to the sides of the socket; I found, as I expected, that though hereby the Syphon was so well clos'd, that the water ran freely through: yet, if I made a hole with the point of a needle, the aire would at so very little an orifice insinuate it self into the cavity of the Syphon, and, thereby gravitating as well within as without, make the water in the legs to fall down into the vessels. And *though*, if I held the point of the needle in the hole I made, and then caus'd one to suck at the longer leg; this small stopple, without any other help from my hand, suffic'd to make the Syphon fit for use: Yet if I remov'd the needle, the aire would (not without some noise) presently get in at the hole, and put a final stop to the course of the water. Nor was I able to take out the needle and put it in again so nimbly, but that the

the aire found time to get into the Syphon; and, till the hole were again stopp'd, render it useless, notwithstanding that the water was by suction endeavour'd to be set a running.

PARA-

PARADOX XI.

That a solid Body, as ponderous as any yet known, though near the Top of the water it will sink by its own weight; yet if it be plac'd at a greater depth than that of twenty times its own thickness, it will not sink, if its descent be not assisted by the weight of the incumbent water.

THIS Paradox, having never been (that I know of) propos'd as yet by any, has seem'd so little credible to those to whom I have mention'd it, (without excepting Mathematicians themselves,) that I can scarce hope it should be readily and generally received in this Illustrious Company, upon less

less clear Testimony, then that of Experience. And therefore, though (if I mistake not) some part of this proposition may be plausibly deduc'd by the help of an Instrument ingeniously thought upon by *Monsieur Paschal*; Yet I shall have recourse to my own Method for the making of it out, for these two Reasons. The one, That a great part of the Paradox must be Explicated, as well as prov'd, by the Doctrine already serled in this paper. The other, That the Experiment propos'd by *Monsieur Paschal*, being to be done in a deep River, and requiring a Tube 20 foot long, whose Bottome must be fitted with a Brass Cylinder, made with an exactness, scarce (if at all) to be hoped for from our Workmen: If I should build any thing on this so difficult an Experiment, (which himself does not affirm to have ever been actually tryed,) I fear most men

would

would rather reject the Experiment as a Chimerical thing, then receive for its sake a Doctrine that appears to them very Extravagant.

Let us then, to imploy in this case also the method we have hitherto made use of, Fill a Glass vessel, *ABCD*, almost full of water; only, in regard that there is a great *Fig. 19.* depth of water requisite to some Circumstances of the Experiment, This last must not be so shallow as those hitherto imploy'd: but a deep Cylinder, or Tube seal'd at one end, whose depth must be at least two or three foot, though its breadth need not be above 2 or 3 Inches; and, to keep it right, it may be plac'd in a vessel of metal or wood, of a size as convenient for such a purpose. This Glass being thus fixed in water, let us suppose *EF*, to be a rod or piece of solid Brass, having

Inch in Diameter, and a fourth or sixth part of an inch in thickness. This Cylinder, being immers'd under water till it be just cover'd by the uppermost Surface of that Liquor, and being let go, must necessarily fall downwards in it; because if we suppose the imaginary Superficies, GH , to pass along the Circle F , which is the lower part of the Brass body, that metal being in itself far heavier then water, the Brass that leans upon the part F , must feel more gravitate upon the said part F , then the incumbent water does upon any other part of the Superficies GH ; and, consequently, the subjacent water at F will be thrust out of place by the descending Body. And because that, in what part soever of the water, not exceeding nine times its thickness measured from the Top of the water AC , the porous Body, BF , shall happen to be; it will be still, by reason of its weight, as lively, as if it were

son of the specifick gravity of the Metal, a greater pressure upon that part of the imaginary Superficies that passes along the bottome of the Body on which the part *F* shall happen to lean, then upon any other part of the same imaginary Superficies; the Brass Body would still descend by vertue of its own weight, though it were not assisted by the weight of the water that is over it. But let us suppose it to be plac'd under water on the designable plain *J K*; and let this plain, which (as all other imaginary plains) is, as well as the real Surface of the water, to be conceiv'd parallel to the Horizon; and let the depth or distance of this plaine, from the uppermost Surface of the water, be (some what) above nine times the thickness of the Brass Body: I say that, in this case, the body would not descend, if it were not press'd downwards by the

the weight of the water it has over it.
 For Brass being but a-
 bout nine times * as
 heavy as water of an e-
 qual bulk to it, the Bo-
 dy *E F* alone would
 press upon the part *F*,
 but as much as a Cy-
 linder of water would,

** The word, above, is added, because indeed the Author, as he elsewhere delivers, did by small scales find Brass to weigh between eight or nine times as much as water, but judg'd it needless to his present Argument, and inconvenient to take notice of the fraction.*

having an equal Basis were 8 or
 9 times as high as the Brass is thick.
 Now as to the other parts of the I-
 maginary surface, *I K*, being press'd
 upon by the incumbent water, which
 But as high above them as the newly
 mention'd Cylinder of water would
 be; there is no reason why the part *F*
 should be depress'd, rather than any o-
 ther part of the Superficies *J K*: But
 because it is true, which we formerly
 taught; namely, that water retains its
 gravity in water; and that too, though
 a body, heavier in *specie* than it, be plac'd
 immedi

immediately under it; it will necessarily happen, That in what part soever the solid body be plac'd; provided it be every way environ'd with the water, it must, for the Reason newly given, be made to move downwards, partly by its own weight; and partly by that of the incumbent water; and must continue to sinck, till it come to the bottom, or some other body that hinders its farther descent.

But in case the water above the solid body did not gravitate upon it, and therefore assist its descent; or, in case that the incumbent water were by some Artifice or other, so remov'd, That none of the lateral water (if I may so call it) could succeed in its place to lean upon the solid; then it will follow, from what we have newly shown, that the solid would be kept suspended. And in case it were plac'd much deeper in the water, as over against the
point

point *L* or *M*; Then, if we conceive the incumbent water to be remov'd or fenc'd off from it, the pressure of the solid alone upon the part *F*, of the imaginary Superficies *L M*, being very much inferior to that of the water upon the other parts of the same Surface, the part *F* would be strongly impell'd upwards, by a force proportionate to the difference of those two pressures. And therefore, since I have found by tryals, purposely made in scales marvellously exact, and with refined Gold, (purer then perhaps any that was ever weighed in water) That Gold, though much the ponderouset of bodies yet known in the world, is not full 20 times as heavy as water of the same Bulk; I kept within compass (as well as employ'd a round number, as they call it) when I said, That nobody (yet known,) how ponderous soever, will subside in water by its own weight alone, if it


were

were so plac'd under water, that the depth of the water did above twenty times exceed the height of the Body; (not to mention here, that though gold and water being weigh'd in the aire, their proportion is above 19 to one, yet in the water, gold does, as other sinking bodies, loose as much of its weight, as that of an equal bulk of water amounts too.)

I was saying just now, that in case the Brazen body were plac'd low enough beneath the Surface of the water, and kept from being depress'd by any incumbent water, it would be supported by the subjacent water. And this is that very thing that I am now to shew by an Experiment.

Let then the Brass body *E F*, be the cover of a brass Valve; *Fig. 20.* (as in the annexed figure) and let the Valve be fastned with some strong and close Cement

to a Glass pipe, OP , (open at both ends) and of a competent length and wideness. For then the Body, EF , being the undermost part of the Instrument, and not sticking to any other part of it, will fall by its own weight if it be not supported. Now then, tying a thred to a Button Q , (that is wont to be made in the middle of the doors of Brass valves) you must, by pulling that string streight and upwards, make the Body, EF , shut the orifice of the Valve, as close as you can; (which is easily and presently done.) Then thrusting the Valve under water, to the depth of a foot or more; the Cement and the sides of the Glass, OP , (which reaches far above the top of the water XT) will keep the water from coming to beare upon the upper part of the body EF ; and consequently the imaginary Surface, VW , (that passes by the lower part of the said


body)

body) will, where it is contiguous thereunto, be press'd upon only by the proper weight of the body EF ; but in its other parts, by the much greater weight of the incumbent water. So that, though you let go the string, (that held the body, EF , close to the rest of the Instrument) the said body will not at all sinck, though there be nothing but water beneath it to support it.

And to manifest that 'tis onely the pressure of the water, of a competent depth, that keeps the solid suspended; if you slowly lift up the instrument towards (XT) the top of the water; you shall find, that, though for a while the parts of the Valve will continue united, as they were before; yet, when once it is rais'd so near the Surface, (as between the plain JK , and XT) that the single weight of EF , upon the subja cent part of the imaginary plain that passes

passes by it, is greater then the pressure of the incumbent water upon other parts of the same plain; that Body, being no more supported as formerly, will fall down, and the water will get into the pipe, and ascend therein, to the level of the External water.

But if, when the Valve is first thrust under water, and before you let go the thred that keeps its parts together, you thrust it down to a good depth, as to the Superficies RS : then, though you should hang a considerable weight, as L , to the Valve EF , (as I am going to shew you a Tryal with a Massy Cylinder of stone broader then the Valve, and of divers inches in length) the surplussage of pressure on the other parts of the plain, VW , (now in RS) over and above what the weight of the body EF , and that of the Cylindrical stone, L , to boot, can amount to, on that part of the Surface which is contiguous

ous to the said body *EF*; will be great enough to press so hard against the lower part of the Valve, that its own weight, though assisted with that of the stone, will not be able to disjoyne them.

By which (to note that by the way) you may see, that though, when two flat and polish'd marbles are joyn'd together, we find it is impossible to sever them without force; we need not have recourse to a *fuga vacui*, to Explicate the cause of their Cohæsiion, whilst they are environ'd by the Aire, which is a Fluid not devoid of Gravity, and reaching above the Marbles no body knows how high.

And to evince, That 'tis only such a pressure of the water, as I have been declaring, that causes the Cohæsiion of the parts of the Valve; if you gently lift it up towards the top of the water, you will quickly find the Brass body,

EF;

E F, drawn down by the stone (*L*) that hangs at it ; as you will perceive by the waters getting in between the parts of the Valve, and ascending into the pipe.

To which I shall only add, what you will quickly see, That, in perfect Conformity to our Doctrin, the pressure of the body, *E F*, upon the subjacent water, being very much increased by the weight of the stone that hangs at it, the Valve needs not, as before, be lifted up above the plain *J K*, to overcome the resistance of the water, being now enabled to do it before it is rais'd near so high.

APPEN-



APPENDIX I.

Containing an Answer to seven Objections, propos'd by a late Learned Writer, to evince, that the upper parts of water press not upon the lower.

AFTER I had, this Morning, made an end of reviewing the foregoing papers, there came into my hands some questions lately publish'd, among other things, by a very recent Writer of Hydrotaticks. In one of which Questions, the Learned Author strongly defends the contrary to what has there been in some places prov'd, and divers places suppos'd.

The Author of these Erotemata asserts,

asserts, That, in consistent water, the upper parts do not gravitate or press upon the lower.

And therefore, I think it will be neither useless, nor improper, briefly to examine here the Arguments he produces. Not useless; because the Opinion he asserts, both is, and has long been, very generally receiv'd; and because too, it is of so great importance, that many of the Erroneous Tenets and Conclusions, of those that (whether professedly or incidentally) treat of Hydrostatical matters, are built upon it. And not *improper*; because our Learned Author seems to have done his Reader the favour to summe up into one page all the Arguments for his Opinions that are dispersedly to be found in his own or others mens Books. So that in answering these, we may hope to do much towards a satisfactory Decision of so important

a Controversie. And, after what we have already deliver'd, our Answers will be so seasonable, that they will not need to be long: The things they are built on having been already made out, in the respective places whereto the Reader is referr'd.

Our Author then maintains, that, in Consistent water, the Superiour do not actually press the Inferiour parts, by the seven following Arguments.

Object. 1. Sayes he, Because else the inferiour parts of the water would be more dense then the Superior, since they would be compress'd and condens'd by the weight of them.

Ans. But if the Corpuscles, whereof water consists, be suppos'd to be perfectly solid & hard; the inferior Corpuscles may be press'd upon by the weight of the superior, without being compress'd or condens'd by them. As it would happen

happen, if Diamond dust were lay'd together in a tall heap : For though the upper parts, being heavy and solid Corpuscles, cannot be deny'd to lean and press upon the lower ; yet these, by reason of their Adamantine hardness, would not be thereby compress'd. And 'tis possible too ; that the Corpuscles of water, though not so perfectly hard, but that they may a little yield to an extream force, be solid enough not to admit from such a weight, as that of the incumbent water ; (at least in such small heights as observations are wont to be made in,) any compression, great enough to be sensible ; As, besides some Tryals I have formerly mention'd in another place, those made in the presence of this Illustrious Company seem sufficiently to argue ; viz. That water is not sensibly compressible by an ordinary force. And I find not, by those that
make

make the Objection, that they ever took pains to try, whether in deep places of the Sea, the lower parts are not more condens'd then the upper: nor do I see any absurdity, that would follow from admitting them to be so.

Object. 2. Our Authors second Argument is, *Because Divers feel not, under water, the weight of the water that lyes upon them.*

Ans. But for Answer to this Argument, I shall content my self to make a reference to the ensuing Appendix, where this matter will be considered at large; and where; I hope, it will be made to appear, that the phenomenon may proceed, partly from the firm Texture of the Divers body, and partly from the nature of that pressure which is exercis'd against bodies immers'd in fluids; which, in that case, (as to sense) presses every where equally, against all
the

the parts of the body, expos'd to their Action.

Object. 3. The third Argument is, That ev'n the slightest Herbs growing at the bottom of the water, and shooting up in it to a good height, are not oppress'd or lay'd by the incumbent water.

Ans. But the Answer to that is easie, out of the foregoing Doctrine. For the Plants, we speak of, sustain not the pressure of the water above them by their own strength; but by the help of the pressure of water that is beneath: which being it self press'd by the water that is (though not perpendicularly over it) superior to it, presses them upwards so forcibly, that if they were not by their Roots, or otherwise fastned to the ground, they, being in specie lighter then water, would be buoy'd up to the top of the water, and made to float; as we often see that weeds do, which storms, or
other

other accidents have torn from their native soyle.

Object. 4. A fourth Objection is this, That a heavy Body ty'd to a string, and let down under water, is supported, and drawn out with as much ease, as it would be if it had no water incumbent on it; nay, with greater ease, because heavy bodies weigh less in water then out of it.

Ans. But an Account of this is easie to be rendred out of our Doctrine; For, though the water incumbent on the heavy body do really endeavour to make it sinck lower, yet that endeavour is rendred ineffectual, to that purpose, by the equal pressure of the water upon all the other parts of the Imaginary surface, that is contiguous to the bottom of the immers'd body. And that pressure upon the other parts of that suppos'd plain, being equal not only to the pressure of the pillar of water,

ter,

ter, but to that pillar, and to the weight of as much water as the immers'd body fills the place of; it must needs follow, That not only the hand that sustains the body, should not feel the weight of the incumbent water, but should be able to lift up the Body more easily in the water, then in the aire. But though the pressure of the water incumbent on the stone can not, for the reason assign'd, be felt in the case propos'd; yet if you remove that water, (as in the Experiment brought for the proof of the last Paradox,) it will quickly appear by the pressure against the lower part of the heavy body, and its inability to descend by its own weight, when it is any thing deep under water; it will (I say) quickly appear, by what will follow upon the absence of the Incumbent water, how great a pressure it exercis'd upon the stone whilst it lean'd on it.

Object.

Object. 5. The fifth Argument is propos'd in these words ; *Because a Bucket full of water , is lighter in the water, then out of it ; nor does weigh more when full within the water , then when empty out of it ; nay it weighs less, for the reason newly assign'd (in the fourth Objection ;) therefore the water of the Bucket, because it is within water, does not gravitate, nor consequently press downwards ; either the Bucket, or the water under the Bucket.* This is the grand and obvious Experiment, upon which the Schools, and the generality of Writers, have very confidently built this Axiom: *That the Elements do not gravitate in their proper place ; and particularly, that water weighs not (as they speak) in its own Element.*

Ans. What they mean by proper or natural place, I shall not stand to examine, nor to enquire whether they can prove, that water or any other sub-lunary

lunary body possesses any place, but upon this account, that the cause of gravity, or some other movent, enables it to expel other contiguous Bodies (that are less heavie or less moved,) out of the place they possess'd before; and gives it an incessant tendencie, or endeavour towards the lowermost parts of the Earth.

But as to the Example propos'd, its very easie to give an account of it. For suppose $A B C D$, to be a Well; wherein, by the string $E F$, the Bucket is suspended under water, and has its Bottom contiguous to the imaginary plain $I K$. If now we suppose the Bucket to consist only of wood, lighter then water, it will not only not press upon the hand that holds the Rope at E , but will be buoy'd up, till the upper parts of the Bucket be above the top of the water; because the wood, whereof the Bucket is made, being lighter in specie then

then water, the pressure of the water in the Bucket *G*, and the rest of the water incumbent on that, together with the weight of the Bucket it self, must necessarily be unable to press the part *H* so strongly, as the other parts of the imaginary plaine *I K* are press'd by the weight of the meer water incumbent on them. But if, as tis usual, the Bucket consists partly of wood, partly of iron; the Aggregate may often indeed be heavier then an equal bulk of water: But then the hand, that draws up the Bucket by the Rope *FE*, ought not, according to our Doctrine, to feel the weight of all the Bucket, much less that of the water contain'd in it. For though that aggregate of wood and iron, which we here call the Bucket, be heavier then so much water; yet it tends not downwards with its whole weight, but only with that surplusage of weight, where-
by

by it exceeds as much water as is equal to it in Bulk; which surplussage is not wont to be very considerable. And as for the water in the Cavity, *G*, of the Bucket, there is no reason why it should at all load the hand at *E*, though really the water both in the Bucket and over it do tend downwards with their full weight; because that the rest of the water, *LI*, and *MK*, do full as strongly press upon the rest of the imaginary Superficies *IK*, as the Bucket and the incumbent water do upon the part *H*: and consequently the bottom of the Bucket is every whit as strongly press'd upwards by the weight of the water, upon all the other parts of the plain *IK*; as it tends downwards, by virtue of the weight of the Incumbent water, that is partly in the Bucket, and partly above it; and so these pressures ballancing one another, the hand that draws the

P Rope

Rope at B, has no more to lift up then the surplusage of weight; whereby the empty Bucket exceeds the weight of as much water as is equall in bulk (I say, not to the Bucket as 'tis a hollow Instrument, but) to the wood and iron whereof the Bucket consists.

And because this Example of the lightness of fill'd Buckets within the water has for so many Ages gain'd credit to, if it have not been the only ground of, the assertion, That water weighs not in its own Element, or in its proper place; I shall add (though I can scarce present it to such a *company* as this without smiles) an Experiment that I made to convince those, that were, through unskilfulness or prejudice, indispos'd to admit the Hydrostatical account I have been giving of the phenomenon. I took then a round wooden Box, which I substituted in the room of a Bucket; and (having fill'd it with melted Butter,

Butter, into which, when it was congeal'd, some small bits of lead were put, to make it a little heavier than so much water,) I caus'd a small string of twin'd silk to pass through two small holes, made in the opposite parts of the upper edge of the box, and to be suspended at one end of the beam of a pair of Gold-Smiths Scales; and then putting it into a vessel full of water, till it was let down there, to what depth I pleas'd, it appear'd that not only the least endeavour of my hand would either support it, or transport to and fro in the water, or draw it up to the top of it; and this, whether the box were made use of, or whether the butter and lead alone, without the box, were suspended by the silken string: but (to evince, that it was not the strength of my hand, or the smallness of the immers'd body, that kept me from feeling any considerable resistance)

stance,) I cast some grains into the scale that hung at the other end of the above mention'd Beame, and presently rais'd the Lead and Butter to the surface of the water. So that unless the Schoolmen will say that the butter & lead were in their own Element; we must be allow'd to think, that the easie sustentation, and elevation of the box, did not proceed from hence, That those bodies weigh'd not because they were in their natural place. And yet in this case, the effect is the same with that which happens when a bucket is drawing out of a well.

And, to manifest that 'twas the pressure of the water against the lower part of the surface of our suspended body, that made it so easie to be supported in the water, or rais'd to the top of it; I shall add, that though a few grains suffic'd to bring the upper surface of the butter to the top of the water:

yet

yet afterwards there was a considerable weight requisite, to raise more & more of its parts above the waters surface ; & a considerabler yet, to lift the whole body quite out of the water. Which is very consonant to our Doctrine. For, suppose the bucket to be at the part *N*, half in and half out of the water : the hand or counterpoise, that supports it in that posture, must have a far greater strength then needed to sustain it, when it was quite under water ; because that now the imaginary plain *PQ*, passing by the bottom of the bucket, has on its other parts but a little depth of water, as from *L* to *P*, or *M* to *Q*, and consequently the bottom of the bucket, *H*, will scarce be press'd upwards above half as strongly as when the bucket was quite under water. And if it be raised to *O*, & consequently quite out of the water ; that liquor reaching no longer to the bottom

of the bucket, can no longer contribute to its supportation; and therefore a weight not only equal, but somewhat superiour to the full weight of the bucket, and all that it contains, (being all suppos'd to be weigh'd in the aire,) will be necessary to lift it clear out of the water.

But to dwell longer on this subject cannot but be tedious to those that have been any thing attentive to the former Discourses. I proceed therefore to our Authors sixth Argument, which is,

Object, 6. That Horse-hairs, which are held to be of the same gravity with water, keep whatever place is given them in that Liquor; nor are depress'd by the weight of the super-incumbent water.

Answ. Whether the matter of fact be strictly and universally true, is scarce worth the examining, especially since we find the difference in point of specifick

cifick gravity; betwixt moft Horfe-
haires, and moft waters, to be inconfi-
derable enough. But the phenomenon,
fupposing the truth of it, is very eafily
explicable, according to the Doctrin
above deliver'd. For fupposing in the
laft Scheme the body, R , to be bulk for
bulk exactly equiponderant to water;
tis plain there is no reason why that
body fhould prefs the part S , of the
imaginary Superficies $I K$, either
more or lefs then that part S would
be prefs'd, if, the body R being anni-
hilated or remov'd, it were fucceed-
ed by a parcel of water of juft the
fame bulk and weight. And confe-
quently, though all the water directly
above the folid R do really lean upon
that body, and endeavour to deprefs it;
yet that endeavour being refifted by an
equal and contrary endeavour, that
proceeds (as we have been but too of-
ten faine to declare) from the prefure

exercis'd upon the other parts of the Superficies, I K, by the water incumbent on them; the body, R, will be neither depress'd nor rais'd. And its case being the same in what part of the water soever it be plac'd, provided it be perfectly environ'd with that Liquor; it must keep in the water (which in this whole Discourse we suppose to be Homogeneous as to gravity) the place you please to give it.

And, (to add That on this occasion) though Mathematicians have hitherto contented themselves to prove, that in case a Body could be found or provided, that were exactly equiponderant to water, it would retain any assignable place in it; yet the Curiosity we had, to give an Experimental proof of this Truth, at length produc'd some glass Bubbles, which some Gentlemen here present have not perhaps forgot, that were (by a dexterous hand we employ'd

employ'd about it) so exquisitely pois'd, as, to the wonder of the Beholders; to retain the places given them, sometimes in the middle, sometimes near the top, & sometimes near the bottom of the water (though that were Homogeneous) for a great while, till some change of consistence or gravity in the water, or some of its parts, made the bubble rise or fall.

The Application of this, to what has been objected concerning Horse-hairs, being too easie to need to be insisted on, there remains to be dispatched out Authors seventh and last Argument, which is this.

Object. 7. That, otherwise, all the inferiour parts of the water would be in perpetual motion, and perpetually expell'd by the Superior.

Ans. But if, by the inferior parts, he means, such portions as are of any considerable bulk; the Answer newly made to the last objection (where we shew'd

shew'd that the body, R, would retain its place any where in the water, and consequently near the bottome) will shew the invalidity of this Objection. And unless we knew of what bignesse and shape the Corpuscles of water are, it would perhaps be to little purpose to dispute how far it may be granted, or may be true in the particles that water is made up of. Only this I shall add, That, whereas this Learned Authour mentions it as an absurdity, that the lower parts of water should be in perpetual motion: And Stevinus himself, in the beginning of his Hydrostatical Elements, seems to me to speak somewhat inconsiderately of this matter; and though, as I lately said, I allow such sensible bodies, as those whose gravity in water Writers are wont to dispute of, to be capable of retaining their places in water, and if they be in specie equiponderant to it. Yet I

I am

am so far from thinking it absurd, that the inferior Corpuscles of water should be perpetually in motion; that I see not how otherwise they could constitute a Fluid body. That restless Motion of their parts, being one of the generallest Attributes of Liquors; and being, in water, though not immediately to be *seen*, yet to be easily *discover'd* by its Effects: As, when Salt, being cast into water, the aqueous parts that are contiguous to it, and consequently near to the bottom, do soon carry up many of the saline ones, to the very top of the water; where, after a while, they are wont to disclose themselves in little floating grains of a Cubical shape.

But, of this restless motion of the parts of Liquors having professedly treated elsewhere already; I shall add nothing at present: But rather take

*In the History of
fluidity & firmness*

notice

notice of what our Authour subjoyns to the last of his Arguments, (as the Grand thing which they suppose) in these words, *Ratio porro, a priori, hujus sementia videtur esse, quia res non dicitur gravitare nisi quatenus habet infra se Corpus levius se in specie.* The erroneousness of which conceit, if I should now go about solemnly to evince; I as well fear it would be tedious, as I hope it will be needless to those, that have not forgot what may concern this subject in the former part of the now at length finish'd discourse; and especially where I mention those Experiments, which show, That neither a stone, nor Gold it self, when plac'd deep under water, would sinck in it, if the Superiour water, that gravitates on it, did not contribute to its depression.



APPENDIX II.

Concerning the Reason why Divers, and others who descend to the Bottome of the Sea, are not oppress'd by the weight of the incumbent water.

Amongst the difficulties that belong to the Hydrostaticks, there is one which is so noble, and which does still so much both exercise and pose the wits of the Curious, That perchance it will not be unacceptable, if to the former Experiments we add, by way of Appendix, one that may conduce to the solving of this difficult problem; viz. Why men, deep under water, feel no inconvenience by the pressure of so great

a weight of water as they are plac'd under ?

The common Answer of Philosophers and other Writers to this puzzling Question, is, That the Elements do not gravitate in their own proper places ; and so, water in particular has no gravitation upon water, nor consequently upon bodies every way surrounded with water. But that this Solution is not to be admitted, may be easily gather'd from our proofs of the first Paradox, and from divers other particulars, applicable to the same purpose, that may be met with in the foregoing papers.

A famous VVriter, and, for ought I know, the Recentest (except *Monsieur Paschal*) that has treated of Hydrostaticks, having rendred this Reason of the Phenomenon.

[The Superior parts of consistent water (as he speaks) press not the inferior, unless

less beneath the inferior there be a Body lighter in specie then water; and therefore, since a humane Body is heavier in specie then water, it is not press'd by the incumbent water, because this does not endeavor to be beneath a humane Body.] He subjoyns, contrary to his Custome, this confident Epiphonema, *Qui aliam causam hujus rei assignant, errant & alios decipiunt.*

But, by his favour, notwithstanding this confidence, I shall not scruple to seek another Reason of the Phenomenon. For I have abundantly prov'd, that (contrary to the Assertion on which his Explication is built) the upper parts of water press against the lower, whether a body heavier or lighter in specie then water be underneath the lower. And, the contrary of which being the *veritas* in this Controversie, perhaps the matter may be somewhat cleared, by mentioning here a distinction,

on, which I sometimes make use of. I consider then a body may be said to gravitate upon another body in two senses. For sometimes it actually sincks into, or gets beneath the body that was under it, as a sincking stone gravitates upon water, and which I call *Prævalent, or successful Gravitation*; & sometimes it does not actually, at least not visibly descend, but only exercises its gravitation by pressing against the subjacent body that hinders its descent; as when a Woman carries a Paile of water on her head, though the weight do not actually get nearer the Center of the Earth; yet actually presses with its whole gravity upon the Womans head, and back, and other subjacent parts that hinder its actual descent; and according to this Doctrine I cannot admit our Authors reasoning, that because a mans body is bulk for bulk heavier then water, therefore the wa-
ter

ter does not endeavour to place its self beneath it. For water, being a heavy body, derives from the cause of its gravity, (what ever that be) an incessant endeavour towards the Center of the Earth; nor is there any Reason, why its happening to be incumbent on a body heavier *in specie* then it self, should destroy that endeavour. And therefore, though it may be said that the water does not endeavour to place it self beneath a humane body, because indeed an inanimate Liquor cannot properly be said to act for this or any other end; yet the water being a heavy body, tends continually towards the lower part of the Earth; and therefore will get beneath any body that is plac'd betwixt it and that, (without regard whether the inferior body be heavier or lighter *in specie* then it self) as far as the degree of its gravity will enable it; nor would it ever rest, till

it have reach'd the lowermost parts of the Earth, if the greater ponderousness of the earth and other heavy bodies did not hinder, (not its endeavour downwards, nor its pressure upon subjacent bodies, but only) its actual descent.

This Learned Author himself tells us, (as well as *Stevinus*, and others, that have written of the *Hydrostaticks*, unanimously teach,) that if the bottom of a vessel be parallel to the Horizon, the weight of water, that rests upon it, is equal to a pillar of water, having that bottome for its Basis, and for its height a perpendicular reaching thence to the uppermost Surface of the water. Non is it reasonable to conceive that there will be any difference in this pressure of the incumbent water, whether the bottom be of Deale that will swimme, (or of Box that will sink in water; or to speak more generally, whether it be of Wood, is *specie* lighter then water,

or of Copper, or some other Metal; that is *in specie* heavier then it. And since water, being not a solid Body, but a fluid, consists (as other fluids) of innumerable Corpuscles, that, though extreamly minute, have their own sizes and figures; And since the pressure of water upon the bottom of a vessel is proportionate to its perpendicular height over the bottom; 'Tis manifest, that the upper Corpuscles press the bottom as well as the lower; which, since they cannot do immediately, they must do by pressing the intermediate ones. And I have already shown (discoursing one of the former Paradoxes,) that the Superior parts of water do not onely presse those that are directly under them, but communicate a pressure to those that are aside of them, and at a distance from them.

And if it be objected, That water

endeavours to get beneath a Bottome of Glafs Vessels, or other bodies heavier *in specie* then its self, because under that bottome there is aire, which is a body lighter *in specie* then water: I say, that this is precarious; for the indisputable gravity of the water is alone sufficient to make it always tend downwards, (though it cannot always move downwards) what ever body be beneath it. And who can assure the makers of this Objection, That there are not beneath even the bottome of Rivers, or of the Sea, (where yet they say water is consistent, and rests as in its own place,) vast spaces replenished but with aire, fumes, or fire, or some other body lighter then water? For, (not to mention that the Cartesians take the Earth we tread on, to be but a thin Crust of the Terrestrial Globe, whose inside, as farre as the Center, is replenish'd with a subtile fluid matter,

matter, like that whereof the Sunne consists.) We know that in some places, as particularly at a Famous Coal-mine in *Scotland*, there are great Cavities that reach a good way under that ground that serves there for a bottom to the Sea: So that, for ought these Objectors know, ev'n according to their own Doctrine, the water ev'n in the Sea, may endeavour to get beneath a body heavier *in specie* then it self.

But, for my part, I cannot but think, that, to imagine the water knows, whether or no there be aire or some lighter body then its self beneath the body it leans on, and the superior parts do accordingly exercise or suspend their pressure upon the inferior; is to forget that it is a heavy Liquor, and an inanimate Body.

Another Solution there is of this Hydrostatical problem, we have been

discourſing of, which I met with in a Printed Letter of Monsieur Des Cartes, in theſe terms.

Je ne me, &c. I remember not what *ſome time* reaſon 'tis that Stevinus gives, *lettre 32.*

why one feels not the weight of water, when one is under it: but the true one is, that there can no more of water gravitate upon the body that is in it, or under it, then as much water as could deſcend in caſe that body left its place.

Fig. 22. Thus for Example: If there were a Man in the Barrel, B, that ſhould with his Body ſo ſtop the hole, A, as to hinder the waters getting out, he would feel upon himſelf the weight of the whole Cylinder of water, A B C, of which I ſuppoſe the Baſis to be equal to the hole A; for as much as if he ſunk down through the hole, all the Cylinder of water would deſcend too; but if he be a little higher, as about B, ſo that he does no longer hinder the water from running

out

out at the hole A, he ought not to feel any weight of the water which is over him, betwixt B and C, because if he should descend toward A, that water would not descend with him, but contrariwise a part of the water which is beneath him towards A, of equal bulk to his Body, would ascend into its place: so that in stead of feeling the water to press him from the Top downward, he ought to feel that it buoys him upward from the bottome; which by Experience we see.

Thus far this subtil Philosopher: for whose Ratiocinations though I am wont to have much respect, yet I must take the liberty to confesse my self unsatisfy'd with this. For having already sufficiently prov'd, That the upper parts of water press the lower, and the bodies plac'd beneath them, whether such bodies be lighter in specie then water or heavier; we have subverted the Foundation, upon

which Monsieur Des-Cartes's ingenious, though unsatisfactory, Explication is built. And yet I shall add *ex abundanti*, That supposing what he says, That in case the solid *B* should descend towards *A*, the incumbent water would not descend with it, but a part of the subjacent water, equal in bulk to the solid, would ascend, and succeed in its room; yet that is but accidental, by reason of the Steadiness and fulness of the Vessel. And though indeed the Superior water cannot actually descend upon the depression of the solid at *B*, if, at the same time while that body descends, an equal bulk of water succeeds in its place: Yet both the solid about *C*, and the water that succeeds it, do, in their turns, hinder the descent of the Superior water; which therefore must graitate upon which soever of the two it be that actually comes to be plac'd

plac'd directly under it, if there be
 nothing, before the displacing of the
 solid, capable to take away the natu-
 ral gravity, upon whose account the
 water, over B and C, does incessantly
 tend downwards. And though Mon-
 sieur Des Cartes does not so clearly ex-
 press himselfe, whether he supposes
 the hole at A to be stopp'd with some
 other body, when the solid is plac'd a-
 bout B; yet, because he is wont to
 speak consistently, I presume he means,
 that when the solid is remov'd to B,
 the hole at A is otherwise suffici-
 ently stopp'd; I say then, that the
 reason why the solid, which whilst at A,
 sustain'd a great pressure from the in-
 combent water, feels not the weight
 of it, when plac'd at B, is not that
 which Monsieur des Cartes gives, but
 this, That the solid being environ'd
 with water, the subjacent water does (as
 we have often had occasion to manifest)

press

press it upwards, full as strongly (and somewhat more) as the weight of the incumbent water presses it downwards; So that a mans body, in stead of sincking, would be buoy'd up; if, as it is a little heavier, it were a little lighter in *specie* then water. Whereas, when the solid was that alone which cover'd and stop'd the hole, there was a manifest Reason why it should be forcibly thrust downwards by the weight of the incumbent water B C. For, in that case, there was no water underneath it at A, to support the solid; and, by its pressure upward, to enable it to resist so great a weight.

And this, (to hint that upon the by) may perchance help us to guess at the reason of what Geographers relate of the Lake *Asphaltites* in *Judea*, (in case the matter of fact be true,) That this dead Sea (as they also call it) will not suffer any living creature to sink in it.

it. For the Body of a Man (and for ought we know of other Animals,) is not much heavier *in specie* then common fresh water : Now if in this Lake (that stands where *Sodom* and *Gomorrah* did, before those impious Regions were destroy'd by fire from Heaven,) we suppose, (which the nature of the Soyle, and the Sacred Story makes probable enough,) That the water abounds with Saline or Sulphurous Corpuscles; (the former helping the later to associate with the water, as we see in some consisting of salt and oyle, and in Chymical mixtures of Alcalis and Brimstone dissoluble in water) the Liquor may have its gravity so augmented, as to become heavier *in specie* then the body of an animal. For I have learned of a Light Swimmer, that he could hardly begin to Dive in salt water, though he easily could in fresh. And 'tis not difficult to make a Brine

or

or *Lixivium* (which are but Solutions of salt in water,) heavy enough to keep up an egg from sincking. And, not only barely by dissolving a metalline body in a saline *Menstruum*, without otherwise thickning the Liquor, I have brought solid pieces of Amber it self to swim upon it : but I have try'd that certain saline Solutions, which I elsewhere mention ; nay, and a distill'd Liquor, (I us'd defleam'd oyle of *Vi-triol*) without any thing dissolv'd in it, would do the same thing ; by reason of the numerous, though minute, *Corpuscles* of salt and sulphur, that it is bound with.

There remains but one solution more of our *Hydrostatical* probleme, that I think worth mentioning, and that is given by the Learned *Stevinus* in these words,

Omni pressu quo Corpus dolore afficitur, pars aliqua Corporis luxatur ; sed isto pressu

pressu nulla Corporis pars luxatur, isto igitur
 pressu Corpus dolore nullo afficitur.
 Assumptio syllogismi manifesta est, nam si
 pars aliqua, ut caro, sanguis, humor, aut
 quodlibet deniq; mem-
 brum luxaretur, in a-
 lium locum concedat
 necesse esset: atqui lo-
 cus ille non est extra
 Corpus; cum aqua un-
 diquaque equali pressu
 circumfusa sit (quod
 vero pars ima, per II.
 propositionem Hydro-
 staticorum, paulo va-
 lidius prematur supe-
 riori, id hoc casu nul-
 lius momenti est, quia
 tantula differentia par-
 tem nullam sua sede
 dimovere potest) neque
 item intra ipsum Corpus
 concedit, cum istic Cor-
 pore omnia oppleta

Stevinus Hydrostat. Lib. 5.
 pag. 149.

Sed Exempla clarius ita
 intelliges, esse ABCD aqua,
 cujus fundum
 DC, in quo sit. Fig. 23.
 ramentum E ha-
 beat Epistaminum sibi infor-
 tum, cui Dorsum incumbat
 Huiusmodi. Quia cum ita sint, ab
 aqua pondere ipsi insidentia
 nulla pars Corporis luxari
 poterit, cum aqua, ut dictum
 est, undique equaliter
 urgeat.

Si vero ejus veritatem
 explorare libeat, eximito
 Epistaminum, tamque tergitum
 nullave sustinetur,
 ut in locis ceteris, ideoque
 istic tanto pressu afficitur,
 quantum veriti exemplo se-
 cunda propositionis hujus
 demonstratur. Est et ali-
 quantulum efficit columna a-
 qua cujus Basis sit fira-
 mica E, altitudo autem ea-
 dem qua aqua ipsi insiden-
 tis. Quod exemplo propositi
 veritas manifeste decla-
 ratur.

sint,

sint, unde singula partes singulis partibus aqualiter resistunt, namque aqua undique eadem ratione Corpus totum circumstat. Quare cum locus is nec intra, nec extra Corpus sit; absurdum, imo impossibile fuerit, partem ullam suo loco emoveri, ideoque nec Corpus hic afficitur dolore.

This Solution of Stevinus, I esteem preferable by far, to those that are wont to be given of this difficult Probleme: But yet, the Phænomenon seems to me to have still somewhat in it of strange. 'Tis true, that if the Question were only that which some put, viz. Why the body of a Diver, when it is near the bottom of the Sea, is not press'd down by so vast a weight of water, as is incumbent on it; It might be rationally answer'd, That the weight of so much water, as leans upon the body, is not sustain'd by the force of the body it self, but by that of the water which is under it. For,

by

by the Experiments and Explications, we have annexed to some of the foregoing Paradoxes, it appears, That the subjacent water, by its pressure upwards, is able, not only to support the weight of the incumbent water, but so far to exceed it, that it would not only support the immers'd body, and the incumbent water, but buoy up the body, if it were never so little lighter *in specie* then water. And as for what *Stevinus* insinuates, That, when the water presses the body every way, that pressure is not felt, though it would be, in case it press'd upon some parts, and not upon others; I am of the same opinion too; and, to prove it, shall not make use of the example he proposes, in the words immediately following those of his, I just now recited: (For I doubt, that example is rather a supposition, then a try'd thing;) but by an Experiment which may be easily made,

made; and has diverse times been so, in our Pneumatical Engine. For, though the aire be a heavy fluid, and though, whilst it uniformly presses the whole superficies of the body, we feel not the pressure of it. And though, for this reason, you may lay the palm of your hand upon the open orifice of a small brass Cylinder, apply'd to the Engine instead of a Receiver, without any hurt; Yet when, by pumping, the aire that was before under the palm of your hand, is withdrawn, and consequently can no longer help to support your hand, against the pressure of the external and incumbent aire; the external aire will lean so heavy upon the back of your hand, that you will imagine some ponderous weight is lay'd upon it. And I remember by such an Experiment, I have not onely had my hand put to much pain, but have had the back of it so bent downward, as if it were going to be broken.

But

But though such considerations, as these, may much lessen the difficulty of our phenomenon, whose cause is inquired into; Yet still it seems somewhat odd to me, That (since 'tis evident from the nature of the thing, and by *Stevinus's* his confession, that there is a vast pressure of water against every part of the body, whose endeavour tends inward,) so exceedingly forcible a pressure, (which thrusts, for instance, the Muscles of the Arms and Thighs against the Bones, the Skin and Flesh of the Thorax against the Ribs,) should not put the Diver to any sensible pain; As I find not (by one that I examin'd) that it dos; (Though this man told me, he stay'd a good while at the depth of betwixt 80 and 100 foot under the Sea water, which is heavier then fresh water;) For, that which *Stevinus's* Explication will only shewis, That there must be no mani-

R

fest

fest dislocation of the greater parts of the Body ; whereas the bare compression of two small parts, one against another, is sufficient to produce a sense of pain.

But it seems, the Texture of the bodies of Animals is better able to resist the pressure of an every way ambient fluid ; then, if we were not taught by experience, we should imagine. And therefore, to satisfy those that (secluding the Question about the sense of pain,) think it an abundantly sufficient Argument, (to prove, that bodies immers'd under water, are not compress'd by it,) That Divers are not oppress'd, and ev'n crush'd, by so vast a load of water, (amounting, by *Stevinus's* computation ; to many thousands of pounds) as is incumbent on them. We will add, that though an Experiment, propos'd by *Monfieur Paschal* to this purpose, were such, that

that at first sight I said that it would not succeed, (and was not upon tryal mistaken in my conjecture ;) yet it gave me the occasion to make another, which will, I hope, fully make out the thing I design'd it for.

The Ingenious Monsieur *Paschal* would perswade his Readers, that if into a glais Vessel, with luke-warm water in it, you cast a flie ; and, by a Rammer, forcibly press that water, you shall not be able to kill, or hurt the flie. VVhich, says he, will live as well, and walk up and down as lively, in luke-warme water, as in the aire. But, upon tryal with a strong flie, the Animal was (as we expected,) presently drowned, and so made moveless, by the luke-warm water.

Wherefore we substituted another Experiment, that we knew would not only succeed, (as you will presently see it will do,) but teach us how great a

pressure the included Animal must
 have been expos'd to. VVe took then
 a somewhat slender Cylindrical
 Fig. 24. pipe of Glafs, seal'd
 at one end, and open at the
 other; and to this we fitted a Ram-
 mer, which (by the help of some
 thongs of soft leather, that were care-
 fully wound about it) did so exactly
 fill the pipe that it could not easily be
 mov'd to and fro; and would suffer
 neither water, nor aire, to get by be-
 twixt it, and the internal surface of the
 Glafs. VVe also provided some small
 Tad-poles (or *Gyrini*) about an Inch
 long or less; which sort of Animals
 we made choice of before any other,
 partly because they could, by reason of
 their smalness, swim freely to & fro in
 so little water as our pipe contain'd; &
 partly because those Creatures, being
 as yet but in their Infancy, were
 more tender, and, consequently, far
 more

more expos'd to be injur'd by compression, then other Animals of the same Bulk, but come to their full age and growth, would be, (as indeed such young Tad-poles are so soft and tender, that they seem, in comparison to the bigger sort of flies, to be but organiz'd Gelly.) One of these Tadpoles being put into the water, and some Inches of aire being left in the pipe, for the use anon to be mention'd ; the water and aire, and consequently the Tadpole, were by the intrusion of the plug or rammer, with as great a force as a man was able to imploy, violently compress'd ; and yet, though the Tadpole seem'd to be compress'd into a little less Bulk then it was of before, it swam freely up and down the water, without forbearing sometimes to ascend to the very top, though the Instrument were held perpendicular to the Horizon, Nor did it clearly appear to

us, That the little Animal was injur'd by this compression ; and most manifest it is, he was not crush'd to death, or sensibly hurt by it.

And having repeated this Experiment several times, & with Tadpoles of differing ages ; we may, I presume, safely conclude, That the Texture of Animals is so strong, that, though water be allowed to weigh upon water, yet a Diver ought not to be oppress'd by it : Since, whether or no water weighs in water, 'tis manifest that in our Experiment, the water, and consequently the Tadpole, was very forcibly by an External Agent compress'd betwixt the violently condens'd aire, and the rammer. And, by the notice we took of the quantity of aire before the compression began, and that to which it was reduc'd by compression ; The moderate estimate we could make, was, That it was reduc'd into an eighth, or
sentb

tenth part of it's former space ; and so (according to what we have elsewhere prov'd) the pressure that was upon the aire, (and consequently upon the water , and the included Tadpole,) was as great as that of a Cylinder of water of above 200 if not 300 foot high. And yet all this weight being unable to oppress, or so much as manifestly to hurt, the tender Tadpole (which a very small weight would suffice to have crush'd, if it prest only upon one part of it, and not upon the other) we may thence learn the Truth of what we have been endeavouring to evince: That though water be allowed to press against water, and all immers'd Bodys ; yet a Diver may very well remaine unoppress'd at a great depth under water, as long as the pressure of it is uniforme against all the parts expos'd thereunto.

F I N I S.

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fig: 1



pag 50. 69

fig: 2

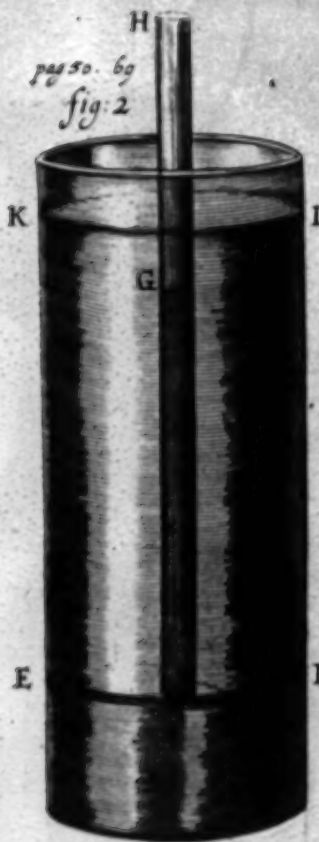


fig 5 pag 24.



fig: 6



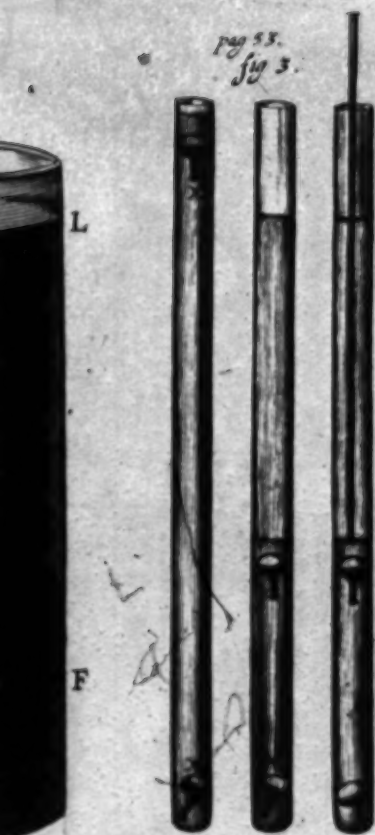


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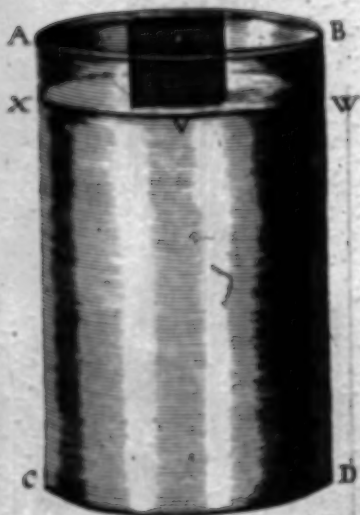


fig 7 pag 119





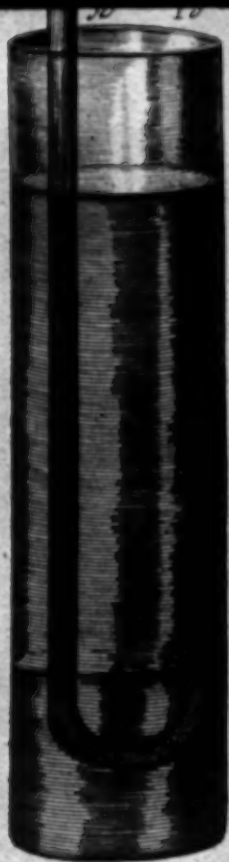
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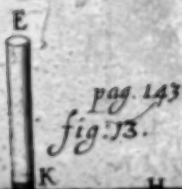
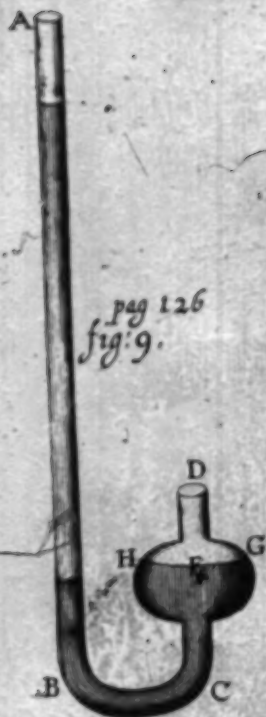
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fig. 11.



K

A

fig. 12. pag. 137.



fig. 16.

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fig. 15. pag. 161



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fig. 13.



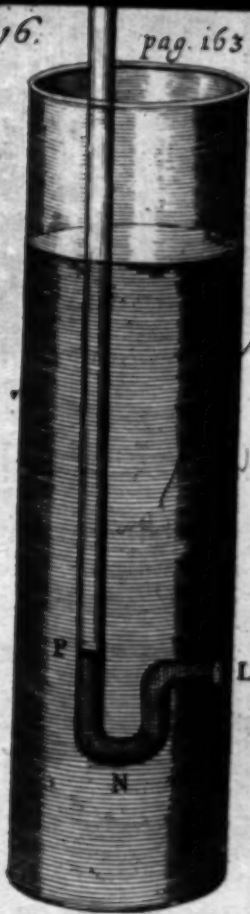
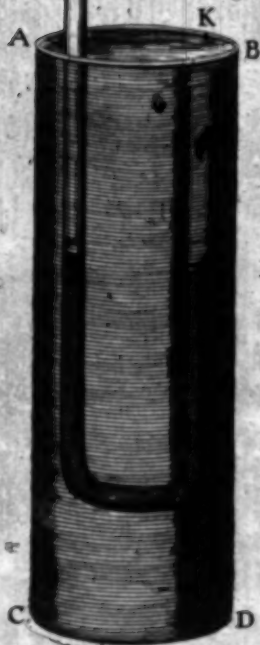
fig. 14. pag. 150



fig. 16.

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fig. 15. pag. 161



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fig: 17.



fig: 18.

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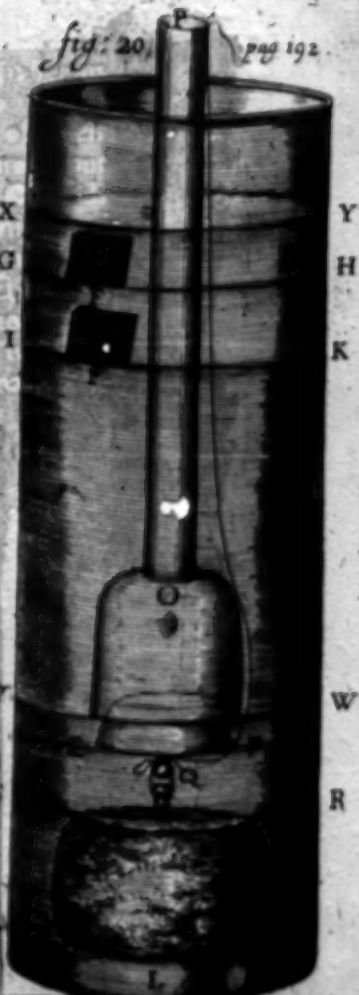
pag 207.
fig: 21.



fig: 19. pag 186



fig: 20. pag 192.





even to the true God
 the other Party not
 order wherein the
 and hope by Resistance and Obedience
 laments to provoke her self Affliction
 not to blame the Murderer
 then than her Ravisher

pag 237
 fig: 23.



pag 244
 fig: 24.

